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COURSE OF LECTURES

ON

DISEASES OF THE SKIN;

THEIR

HISTORY, PATHOLOGY, AND TREATMENT.

Delivered in the Middlesex Hospital, during the Summer Session, 1842, *by*

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LECTURE XVII.

DOES vaccine lymph lose its protective powers by indefinite transmission? This is a question which has been much and warmly agitated, but is unsupported by any very conclusive evidence. It has been proved that the lymph introduced into use by Jenner is still capable of producing the perfect Jennerian pock, and that the constitutional influence of the inoculation is complete. If, then, we associate this observation with the multifarious causes of imperfect vaccination which are known to exist, we may, I think, attribute any loss of power of the lymph rather to the latter cause than to any supposed deterioration, the effect of time. Indeed, it is consistent with our knowledge of physiological phenomena to expect that a specific poison introduced into the animal system shall excite a specific action, followed by certain specific results; and that the reproduction of virus, which is the essence of this process, shall furnish a poison identical with that introduced into the system, and capable of exciting perfectly similar effects. There is nothing in this process that indicates a probability of change in the phenomena, however frequently it may be repeated. Notwithstanding, the opinion of a decrement in the power of the vaccine lymph is entertained by many practitioners, and has given birth to several ingenious and important suggestions for improving vaccination by the introduction of better lymph.

IMPROVEMENTS IN THE VACCINE LYMPH.

The experiments which have been had recourse to for improving the vaccine lymph are three in number, namely, Recurrence to the Cow, Retro-vaccination, and Variolo-vaccination.

RECURRENCE TO THE COW.

From the variety of epizootic disorders which affect cattle, it is a matter of considerable difficulty to determine the precise form of eruption which represents the true cow-pox. A widely-spreading and destructive disorder of the vesicular kind, the aphtha epizootica, has, for the last few years, been prevalent among cattle. By the inexperienced this disease might be mistaken for the true disease, and the following remarks are made by Mr. Ceeley on other disorders, or "spurious pocks," to which, according to his observations, milch cows are liable:—

"Inflammation and induration, sometimes suppuration of the cutaneous follicles at the base of the teats; small hard knots, cutaneous or subcutaneous, in the same locality, about the size of a vetch or pea, or even larger, which often remain indolent for a time, at length become red, vesicate, enlarge, suppurate, and burst, after attaining, not unfrequently, the size of a walnut or more, occasionally affecting the hands of the milkers, and often the other cows milked in the same shed by the same hands. An eczematous eruption, with intertrigo on the udder and near the roots of the teats. Warty growths of two kinds; one consisting of long, narrow, pendulous, and linear-shaped prolongations, easily removed, and often detached; the other of short, thick, compact, broad elevations, lighter in colour generally than the ground from which they rise, of various sizes, from that of a pea to a horse-bean, frequently very numerous on the teats, where they are often found bleeding and partially detached. The *yellow pock*, a pustular eruption, resembling ecthyma, on the teats and udders, succeeded by thin, dirty-brown or black, irregular crusts. The *bluish or black pock*; bluish or black, or livid vesications on the teats and udders, followed by thin, dirty-brown or black, irregular crusts, and some degree of impetigo on the

interstices, near the basis of the teats. The *white pock*, a highly contagious disease among milch cows and to the milkers, quickly causing vesications and deep ulcerations, often, or almost always, confounded by them with the true vaccine, and certainly not readily distinguishable in all its stages by better informed persons than milkers."

Of the true variolæ vaccinæ Mr. Ceeley remarks, that they are preceded for three or four days by heat and tenderness of the teats and udder, without any apparent constitutional indisposition. On the heated and tender parts red and hard pimples, of the size of a pea or vetch, make their appearance. In three or four days the pimples increase to the size of a horse-bean, and, continuing to enlarge, develop vesicles on their summits. The inflamed parts at this period are exceedingly tender, and milking is both troublesome and dangerous. On their first appearance the vesicles are acuminated, oval, or globular; when broken they give exit to a viscid amber-coloured fluid, and by the collapse of the epidermis present a central depression with marginal induration. From the injury done to the inflamed parts by milking, and the consequent inoculation of the neighbouring surface, the vesicles present every variety of size and period of development; intermingled with the vesicles are several dark-brown or black solid, uniform, embedded crusts, and in situations where the latter have been removed there are raw surfaces, with a slight central slough.

Notwithstanding the difficulties attendant on the discovery of the true vaccine disease, lymph has been procured by several gentlemen, both in this country and on the continent. Of the former, I mention, especially, the names of Mr. Estlin, of Gloucestershire; Mr. Fox and Mr. Sweeting, of Dorsetshire; and Mr. Ceeley, of Buckinghamshire; and of the latter, M. Saunoy and M. Bousquet, in France, and S. Griva, in Turin. The effects produced upon the human subject by the primary lymph I have already detailed in a previous lecture. I laid particular stress on the advantages resulting from the inoculation of infants, from their insusceptibility of morbid poisons; and I mentioned the indisposition which pervades the several genera of animals to take on the peculiar diseases of another genus, a circumstance which is particularly observable in the attempt at transmitting vaccinia from the cow to the human system. That the obstacle, however, is not insurmountable, has been amply demonstrated by experiment, and of all the plans hitherto suggested for improving the vaccine lymph this appears to be the most legitimate, and the best calculated to ensure success.

RETRO-VACCINATION.

A far less satisfactory proposal for improving vaccine lymph, when its powers are supposed to be enfeebled, is to pass it again through the system; in other words, to vaccinate the cow. This operation is attended with much difficulty, for a reason to which I have had frequent occasion to advert, namely, the indisposition of the system of one genus of animals to be acted upon by morbid poisons derived from an animal of a different genus. The experiment has, however, succeeded in the hands of Mr. Ceeley, and that gentleman, by transmitting the resulting virus to children, has been enabled to determine the qualities which the lymph had obtained by its re-passage through the cow.

"My experiments in retro-vaccination," says Mr. Ceeley, "were almost entirely confined to the sturk under twelve months old, and the parts selected for the operation were those in close proximity to the vulva. These animals were easily procured, while milch cows in any number, at least here, are difficult to obtain for such a purpose. The operation was performed either with a lancet or a sharp straight bistoury. The lymph was either dry or liquid, in capillary tubes. No other precautions were taken than excluding the animals from wet and cold immediately after the operation, for a few hours, or perhaps a night. Those of a light colour and with thin skins were generally preferred, but often without avail, scarcely one half of the operations succeeding. Tubercles, nearly or completely lymphless, were often produced; but sometimes every puncture succeeded. In the majority of instances the vesicles ran the normal course, declining on the eleventh day."

In his general observations on the results of retro-vaccination, the same author observes:—"Take an abundance of humanised lymph from one of the finest and most productive vesicles ever seen, and if you succeed in retro-vaccinating the cow, you may perhaps be able to charge scantily only a very few points from a vesicle which excites but trifling topical inconvenience. Vaccine lymph, it is obvious, therefore, in passing from the cow to man, undergoes a change which renders it less acceptable and less energetic on being returned to many individuals of the class producing it; some refuse it altogether. But this reluctance or absolute refusal, this difficult inhibition or total insusceptibility, does not exist to the same extent as observed in the converse of this experiment, viz., in passing primary lymph from the cow to man by the same means. Here we have considerable difficulty, but when we succeed a severer disease is induced. The age of humanised lymph does not seem to influence its reception by the cow. Provided that the lymph be of ordinary activity, and possess the normal qualities, it as likely to succeed in its operation on the animal whether it may have been current in man a few weeks or many years,

and will excite equally perfect and productive vesicles."

Of the effects of retro-vaccine lymph on man, Mr. Ceeley remarks, "Lymph which has passed from arm to arm with the greatest promptness and facility, and produced the finest effects, in its first removes from the costive vesicle of the retro-vaccinated cow, is not so readily absorbed by many individuals as current lymph. It rarely fails, but papulation is retarded, and though the vesicle may attain maturity at the normal average period, the completion of that stage is frequently postponed. The vesicles are often smaller, and the disease really not so well developed as by the stock from which it was derived. But these changes do not appear after the second or third remove. The lymph is restored to its former qualities and produces its former effects."

Let us see what answer Mr. Ceeley returns to the question propounded by himself, namely, "Is the practice of retro-vaccination of any real value as a means of renovating humanised vaccine? I confess that from my limited experience I am unable to discover its advantages, or to admit more than its questionable utility, from one transit through the cow. What humanised vaccine might acquire by repeated and indefinite transmissions, I am not prepared to say. I have no doubt that primary vaccine, passed by artificial means through a series of cows, would lose much of its acrimony, and produce on man a milder, but abundantly active and characteristic disease; but that humanised vaccine, by a similar process, should ever acquire all the attributes of primary vaccine, or become so much *brutalised* as to deserve the epithet of 'renewed,' is more than many are prepared to expect."

VARIOLO-VACCINATION.

Another proposal for renewing the vaccine lymph is founded on the identity of small-pox and cow-pox. I have already discussed the question of identity of these disorders in previous lectures; it is only necessary for me now to explain that the proceeding termed variolo-vaccination consists in inoculating the cow with small-pox virus, and employing the resulting lymph for the inoculation of man. This variolo-vaccine lymph is very effective, but it is questionable how far it may possess advantages over the ordinary current vaccine lymph. The experiment of procuring it is attended with many obstacles, both from the difficulty of procuring suitable subjects, and from the indisposition they present to the reception of the virus, and many failures must naturally be anticipated. Nevertheless, the operation has been successful in the hands of several experimentalists, and Mr. Ceeley, who was so fortunate as to succeed on several occasions, gives the following account of the effects of the lymph when transferred to man:—

The first person subjected to the action of the new lymph was his own assistant, Mr. Taylor, who was in consequence the subject of the following interesting notice:—This gentleman, it appears, while Mr. Ceeley "was engaged in the tedious process of charging points therefrom, punctured the skin of his own hand between the thumb and finger, with the instrument while moist with lymph. On the fourth day afterwards he directed my attention to a hard, deep-red, papular elevation on the spot, stating the cause, and at the same time assuring me that he had been vaccinated in infancy, and had subsequently had modified small-pox. On the fifth day there was a papulo-vesicular elevation, surrounded with a dark red areola, and much uneasiness in the part. In the evening headach and other febrile symptoms appeared, with roseola, and fiery-red papulæ on the face and other parts. On the sixth day a more diffused and lighter areola surrounded the less abrupt elevation, which was now more perfectly vesicular; the constitutional symptoms increased, and the papulæ on the face, neck, trunk, and limbs, exhibited ash-coloured summits, and through a lens appeared to have slight central depressions. On the seventh day it was manifest that the disease had reached its acmé on the previous day; the areola was diminished, the vesicle was more apparent, some of the papulæ presented light straw-coloured summits, and the roseola was declining, with an abatement of the febrile symptoms, and a diminution of the tenderness in the axilla. On the eighth day all these changes were more obvious, although he was not free from headach, the papulæ were more yellow, and some were desiccating, the vesicles were larger but less active, and the areola was comparatively pale. This was evidently modified vaccine in a sanguine habit, with roseola, and vesicular or vaccine eruption."

Out of five children inoculated with the variolo-vaccine lymph in twenty punctures, Mr. Ceeley obtained only six vesicles. In all these cases the punctures inflamed early, but the papular stage was later, and it was not until the sixth, seventh, or eighth day that it became possible to determine the probability of success. The primary constitutional symptoms were slight, while the secondary were proportionate to the extent and character or the areola; thus, in one there was considerable suffering, and an extensive areola, with vomiting and delirium. The following is a successful case of variolo-vaccination:—"Emma Jaycock, aged 14, dark swarthy complexion, thin skin, rather florid; two points of sixth-day lymph and four of eighth-day lymph were inserted into six punctures. On the fifth day four of the papulæ had ash-coloured summits and seemed vesicular, two were doubtful. On

the seventh day there were five small, distinct, reddish-grey or ash-coloured vesicles, one very small. On the eighth day the vesicles were advancing, of unequal size and of irregular form. Here I was forcibly struck with the strong resemblance some of those vesicles bore to those of the eighth day depicted in Jenner's work on the arm of Hannah Excell, which he thought so remarkably like the results of small-pox inoculation. My patient stated that she felt slightly indisposed on the fifth and sixth day, that the axilla was painful on the seventh day, and that she was then giddy and sick, but felt worse on this, the eighth day. On the ninth day the areola commenced, and she complained only of headach; on the eleventh day it was fully developed, when all her symptoms returned in an aggravated form. On the twelfth day it declined, but the turgid vesicles having burst the flimsy cuticle, renewed inflammation and induration, with circumscribed sloughing and ulceration of the skin ensued, and rather deep scars are now visible."

As a type of the effects of this lymph after having passed through several individuals, Mr. Ceeley gives the following example of a case illustrative of the fourteenth remove, in "an infant fourteen months old, florid, plump, and healthy, with a fine, clear, thick, smooth skin. In the majority of instances in propagating from arm to arm distinct papulation was apparent on the second day; on the third it was not only visible but well defined; on the fifth and sixth vesiculation was abundantly obvious, and lymph was often taken on those days. On the seventh day vaccination was frequently performed, and points were often charged; on the eighth the vesicle commonly exhibited a bold, firm, and glistening aspect; between this period and the ninth day the areola generally commenced (but in young infants with tense and sanguine skins it appeared early on the eighth); by the tenth day the vesicle was commonly in its greatest beauty and highest brilliancy, glistening with the lustre of silver or pearl, having the translucency and appearance of crystal, or shining with a pale blue tint, occasionally of a dull white or cream colour, bold and elevated, with a narrow centre and broad margin, or flat and broad in the centre with an acute margin, occasionally not raised above the level of the skin. On this and the eleventh day an extended and generally vivid areola existed, with more or less tension and induration on the integuments; at this time the lymph was frequently pellucid and often perfectly efficient. From the eleventh to the thirteenth day, gradually increasing in many individuals both children and adults, sometimes the entire vesicle, at others only the central parts reflected a blue or slate-coloured tint, from the congested or ecchymosed subjacent adventitious structures, proportionate to the texture and degree of translucency yielded by its desiccating epidermis. On the thirteenth and fourteenth day, particularly on clear skins, moderately thick, the vesicles attained a considerable size, measuring often in their longest diameters six and a half or seven lines, and acquired a light brown centre from commencing desiccation, which was surrounded by a dull margin of dull white or pale dirty-yellow, soft and flaccid, and still possessing fluid contents. During this period, the areola, of a dull red or damask hue, would revive and decline again and again, and even to the sixteenth or eighteenth day, the period to which complete desiccation was frequently protracted. The crust commonly partook of the form of the vesicle; it was often prominent and bold, varying in colour from that of a chesnut to that of a tamarind-stone. It fell generally about the twenty-third or twenty-fifth day, often later."

"The cicatrices were of variable depth and extent. When the vesicles remained unbroken on a thick sanguine skin, they were deep, but on a thin skin shallow, they were not always proportionate in width to that of the vesicle, the smallest vesicle often succeeding the largest vesicle, but the later the crust fell of course the deeper the cicatrix, which on these occasions was often beautifully striated. I need scarcely say that when the vesicles were accidentally broken or spontaneously burst, much mischief ensued, deep sloughing of the skin, &c. Spontaneous bursting did not often occur except in those subjects possessing the before-mentioned obnoxious constitutional and dermic characteristics, upon whom we must always use active lymph with some risk."

"When the lymph in the first remove produced normal vesicles, and as soon as it had passed readily from arm to arm, the constitutional symptoms, though mild, were most commonly well marked. In infants restlessness, fretfulness, and inappetency, about the fifth or sixth day, were very common, sometimes as late as the seventh day, very few escaped feverish symptoms on the ninth and tenth days, many had vomiting and diarrhœa. From childhood up to puberty the primary symptoms from the fifth to the seventh day were unequivocally visible and often complained of; fever, vomiting, delirium, and diarrhœa, were not unfrequently witnessed at the commencement or during the progress of the secondary symptoms. In adults, of course, more complaint was made, headach, chilliness, anorexia, and sometimes thirst, on the fifth or sixth day, increased on the seventh day, with axillary tenderness, but on the ninth and tenth days much general febrile complaint, disinclination and even inability to leave the bed. But in several instances among young children, little or no complaint was made or indicated. All the

members of the same family vaccinated from the same source might be differently affected; one, for instance, would not cease from pastime, occupation, or meals, while another, particularly if older, would be indisposed for several days. Neither the number nor the magnitude of the vesicles seemed to determine the amount of the primary disturbance. If properly developed, small vesicles often gave rise to marked constitutional symptoms, and the most splendid vesicles were often seen with trivial, sometimes scarcely appreciable disturbance."

"The secondary symptoms were often as active with three or four as with six or eight vesicles. Acceleration of the pulse was frequently noticed when no other symptom appeared; both primary and secondary symptoms very commonly showed a remitting type." "In no case, excepting that of Mr. Taylor, was there any attendant eruption."

Other observers besides Mr. Ceeley have succeeded in performing variolo-vaccination, and have recorded the results of their experiments. Dr. Basile Thiele, of Kasan, in South Russia, remarks that the effects produced by this lymph were more intense than those which resulted from ordinary vaccine lymph. Sometimes there were two febrile attacks, the one between the third and fourth day, the other between the eleventh and fourteenth; and these severe effects were not lost until the sixth remove. In one case the lymph produced an attack of true variola, and lymph taken from the pocks of this patient and inoculated in the arm of another reproduced vaccinia. This latter experiment is a marked illustration of the identity of variola and vaccinia.

In concluding the interesting and important subject of vaccination, and the consideration of the defences against small-pox, I may mention a singular statement made by Dr. Lichtenstein, and published in Hufeland's Journal for 1841. The paper in which this statement occurs is entitled, "On the Sources from which Matter preservative against the Small-pox has been derived," and the author asserts that limpid lymph taken from the pustules produced by tartarised antimony and inoculated in a person who has not been vaccinated, occasions a pock that cannot be distinguished from vaccinia. These pocks, he states, are equally protective against small-pox, and the lymph may be transmitted from person to person in the same manner with cow-pox. Dr. Lichtenstein inoculated and reinoculated thirty-one persons from this source, and none of these persons took the small-pox, although they mingled freely with the infected during the epidemic prevalence of small-pox.

The glands, properly so called, separate fluids from the body, and discharge them by a duct. They are all situated on the head or trunk.—*Dr. Alison.*

CLINICAL REMARKS

ON CASES OF

EXCISION OF DISEASED BONE,

DELIVERED AT

KING'S COLLEGE HOSPITAL

BY

MR. FERGUSSON.

Case I.—Excision of a Portion of the Scapula.

Henry Parry, ætat. 28, a sailor, was admitted under Mr. Fergusson October 10, 1842. Twelve months ago, being then in Calcutta, had an attack of what he calls rheumatism of the right shoulder; the parts swelled much, accompanied with great pain, and he was unable to use the arm, which consequently became much emaciated. In the course of a few weeks the swelling and pain partially subsided, becoming, however, more remarkable on the posterior surface of the joint. Fluctuation being felt, an opening, two inches and a half in length, was made, and exit was given to a large quantity of fetid pus. The wound has never entirely closed, and six or seven pieces of bone have been discharged at intervals, the last about three months ago.

There are now two sinuses, one just below and behind the acromion, the other a little lower, from which there is a constant discharge of thin fluid; there is also an opening in front of the shoulder, an abscess having formed in that situation. All movements in the shoulder-joint have ceased, and he has become totally unfit for service. The spine and acromion scapulæ are considerably thickened, and a probe passed along one of the sinuses over these parts grates against bare bone.

18. To-day a free incision was made, which permitted a more complete examination of the spine of the scapula, and with the aid of the gouge and forceps, a portion of necrosed bone was removed from a shell-like cavity in the spine, formed by a deposition of ossific matter around the necrosis. The cavity and wound were stuffed with lint; cold was applied to check the oozing of blood, and on the succeeding day warm water dressings were used over clean charpie, with which the wound was filled daily. Granulations soon occupied the space, and as cicatrisation became complete the relief from pain and facility of moving the shoulder became more and more apparent. The puncture in front of the shoulder showed no disposition to heal, and from time to time, under the impression that there might be a portion of dead bone in this situation, a probe was introduced, which, however, failed to indicate any such condition. At last, after the occurrence of some additional inflammation in this locality, a piece of bone

was detected, and then removed with a small pair of dissecting forceps. In a few days all pain having ceased, and the opening being about closed, the patient was discharged cured.

In this case it was remarked, that there was difficulty in deciding whether the original disease had commenced in the shoulder-joint or actually in the scapula. When the patient first came to the hospital, however, it was evident that it had ceased in the articulation, having ended in anchylosis, but that without surgical interference it was impossible to foretell when it might get well otherwise, for the separation and discharge of diseased or dead portions of bone were processes of great uncertainty. The steps which had been taken showed their advantage by the speedy relief from pain which resulted, and also by the rapidity of the cure. The patient had suffered for twelve months, and years might have elapsed before the dead portion of the scapula had been discharged, if, indeed, that could ever have happened under the circumstances. It was impossible to determine from what part the piece of bone removed from the sinus in front of the joint had separated; it might have come from the head of the humerus, or from some contiguous part of the scapula; but if from the part of the spine of that bone from whence a large portion had been removed, it must have taken a singular course to reach the surface.

CASE II.—DISEASE OF OS CALCIS.—EXCISION.

Jane Gilbert, ætat. 29, admitted November 22, 1842. Five months ago experienced great pain in the left heel and across the instep, and shortly after felt similar sensations in the knee of the same limb. A hard painful swelling formed on the outer side of the heel a little below the malleolus, and a sore broke out on the skin over the part, which has gradually increased till now, when it may be described as an irritable ulcer, with highly inflamed and swollen edges, and a thick slough in the centre. The slightest touch with the probe causes a copious flow of blood, but no bone can be detected. There is a point which may be covered with the tip of the finger over the external condyle of the femur, which is exquisitely painful, and effusion into the joint is conspicuous; patient to be kept in bed; knee and heel to be poulticed.

26. Slough has separated from the sore on the heel, and left the bone exposed.

Dec. 1. An issue with caustic potash formed over the painful part of the knee.

Jan. 29, 1843. Since 1st December ult., pain in the knee has gradually ceased, effusion has been absorbed, and there now only remains a small superficial sore, resulting from the application of the caustic. During the above interval the heel has caused great distress, and at the patient's urgent request Mr. F. has to-day made a crucial incision over the bare portion of bone, and thus scooped it away with the gorget. The wound has been stuffed with lint; cold has been applied, and fomentation ordered for the evening.

Feb. 6. Pain in heel has been considerable since the operation, and the wound has granulated with a copious suppuration. To-day a small portion of bone, somewhat larger than a pin-head, has been detected lying loose in the bottom of the sore, and has been removed with dissecting forceps. Warm water dressings and occasionally unguentum cetacei; a strap of adhesive plaster sometimes drawn across the sore to promote contraction.

21. Pain has gradually ceased, and the sore is now about healed; little tenderness on pressure, and no appearance of further disease either in the heel or knee; health much improved.*

After the operation Mr. F. remarked, that such a proceeding was often of great service in bringing about a speedy cure of an otherwise very tedious disease. He had frequently adopted a similar practice in other cases of the kind, and had succeeded in curing a disease of several years standing in the course of a few weeks. He believed that the advantage of such treatment was not sufficiently understood, and that much benefit might accrue from its more frequent adoption. He had operated in this instance at an earlier period than he thought altogether eligible, but had been induced to do so from the earnest wishes of the patient, and also because he imagined that even then he could afford relief to her sufferings with a fair prospect of a cure. He would have preferred waiting until there was evidence that a portion of bone had become loose, or, at all events, until the acute stages of the disease had seemingly ceased. In the operation he had found the lower and outer margin of the bone in a state of necrosis, but not yet separated from the neighbouring osseous tissue. In scooping it away his principal guide in determining when he had done enough, was the oozing of blood from the sound part of the bone. It was often difficult in these operations on the bones, more especially in caries of the spongy parts, to determine when all the disease had been removed. Sometimes the surgeon being guided by the appearance of the portions removed, some-

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* March 14. Shortly after the last date this patient began to move about the ward. The erect position caused considerable swelling in the foot, and some pain in the vicinity of the cicatrix. A small portion of the wound has not yet healed, but there is no sinus, nor any evidence of diseased bone having been left.—*Rep.* LANCET.

times by the hardness in the parts where the cutting instrument has been last applied, in all instances the appearance of blood being a good test.

CASE III.—NECROSIS OF FEMUR AFTER AMPUTATION; OPERATION.

James Churchill, ætat. 48, a sailor, admitted Jan. 25, 1843. In March, 1838, had his right knee crushed at sea, and was subjected to various modes of treatment until the summer of 1839, when amputation in the thigh was performed at the Dreadnought Hospital Ship, where he remained until November, when he left with the wound still open. A portion of bone had been cut away subsequent to the operation. He stayed at home, continuing to poultice the stump, until February, 1840, when he went into Guy's Hospital, where he was kept two years and seven months. During all this period he suffered great pain in the part; a small portion of bone had been discharged, and a swelling at the under and back part of the stump had been punctured with a knife sixteen different times, a little blood only escaping on these occasions. In September last he was discharged from that institution, and was under the impression that nothing could be done to allay his sufferings. Has for some time been an inmate of the Wandsworth Union Workhouse, and has latterly experienced great and constant suffering, being unable to move or permit the part to be touched without experiencing acute pain. The stump is of great size, patient being large and corpulent; on the end there is an open ulcer through which bare bone can be felt with the probe or finger. The scars of the former punctures are quite visible. There is a small sore on the skin over the trochanter major, which discharges matter. The amputation has been performed about the middle of the thigh.

Jan. 26, 1843. To-day Mr. Fergusson made a free crucial incision on the end of the stump, and raised the soft parts with the knife so as to enable him to expose the end of the femur. He then introduced a pair of strong sequestrum forceps, and with a continued application of some force (applied downwards in the axis of the bone) extracted a sequestrum five inches in length, which, at the lower end, included the whole circumference of the femur, and gradually tapered to a point above. The operation caused great pain, every movement of the sequestrum calling forth screams of agony; the bleeding was copious, but no vessel required a ligature. The wound was stuffed with lint, cold cloths were applied, and warmth was ordered in the evening.

Feb. 27, 1843. Patient required opiates for several nights after the operation. In a few days the tenderness in the stump wore off, he could move himself in bed, and permit the part to be handled without suffering as he did before. Now the wound has nearly closed, he has no pain, and can move about freely. The size of the stump is greatly diminished.

After the operation and at subsequent visits Mr. F. stated that necrosis sometimes followed even the best performed amputations. This result had followed in this instance, but the chief circumstance to be deplored was that the patient had suffered so long when the surgeon had the remedy in his power; it would have been improper to have interfered previously to the loosening of the sequestrum; when that had happened he could not say, but he supposed that a proceeding similar to that which he had followed might have been had recourse to at an earlier period. It had been imagined by some who had seen the case before that a malignant disease existed, and when he himself had been asked his opinion an idea was entertained that a second amputation very high up, possibly at the hip-joint, would be requisite. The history of the case and the condition of the stump led him to conclude that a sequestrum was the cause of all the distress. He had satisfied himself that such it really was, and came to the conclusion that a reasonable attempt might be made to extract the dead portion of bone by making a proper opening at the end of the stump. The result spoke for itself, and afforded a good example of judicious interference on the part of the surgeon, when it had become evident that the sequestrum would never separate by natural means.

CHOLERA FROM EATING BARBEL ROE.

THERE seems to be no doubt that the roe of this fish (the *cyprinus barbus*) has, as Sauvages, Kopp, Marx, &c., have asserted, a poisonous quality, which is especially manifest on children. The symptoms caused by it, which show themselves soon after it has been swallowed, are frequent bilious vomitings, precordial anxiety, diarrhœa with tenesmus, profuse perspirations, and convulsive movements of the limbs. In process of time the features become altered, and hiccup with syncope ensues. Before any violent vomiting comes on, an emetic of ipecacuanha is advantageous; but after this symptom has set in, Trusen recommends the employment of revulsives to the cardiac region, and that lemonade should be given in large quantities. Another authority advises the use of caustic spirit of ammonia, in doses of from fifteen to twenty drops, in a glass of water, every five or ten minutes.—*Hufeland's Journal.* The treatment suitable in this case would probably be identical with that in other cases in which poisonous fish had been taken.

USES OF CONSTIPATION AND ABUSE OF PURGATIVES.

EPILEPSY. INTESTINAL OBSTRUCTION.

To the Editor of THE LANCET.

SIR,—"Il y a," writes Fontanelle, "des mots qui hurlent de surprise et d'effroi de se trouver unis ensemble;" and possibly to the phrase "uses of constipation," with which I head this notice, may seem to apply the remark now quoted of the French philosopher. Nevertheless, I am prepared to maintain, from frequent observation, and even experiment, that, in not a few maladies, a constipated condition of the bowels is advantageous; an opposite condition, unequivocally hurtful.

Among the affections in which a considerable degree of constipation is useful, are certain forms of chronic rheumatism, certain cardiac affections, and incipient phthisis. In acute rheumatism, occurring for the first time, and in young and robust subjects, purgatives are useful or indispensable. But it is otherwise when the disease has assumed the chronic type, and occurs in persons debilitated by the disease itself, or by other causes, or in those beyond mature manhood and merging into age. With such, even moderately free purging is almost sure to aggravate the attack, or to convert merely premonitory symptoms, as slight sensations of chilliness, or fleeting muscular or articular neuralgia, into an actual and general attack. I have met in practice with various instances of such untoward results from an incautious use of purgatives in the circumstances referred to.

Of all the varieties of opening medicine, saline purgatives are those which, when injudiciously used, most disastrously operate in inducing or in aggravating chronic rheumatism. On this point I can speak confidently, from tolerably extensive and from careful observation. I have repeatedly known, in persons *subject* to chronic rheumatism, but for the moment entirely free from it, a severe and protracted paroxysm induced by one or two incautious and scouring doses of saline purgatives. A day or two after the taking of these, the patients have felt their sensibility of cold remarkably increased; slight rigors followed, and in from four-and-twenty to forty-eight hours, general rheumatism has developed itself, which, in the course of the five or six subsequent days, has gradually augmented in severity. It is not altogether an easy matter to account for this peculiar action of saline purgatives in the affection under consideration. I am disposed to suspect that it is principally through the sedative influence which saline purgatives produce on the nervous system. The first effect of saline purgatives is to produce a serous discharge from the bowels, and consequently to diminish the proportion of serum in the blood; relatively increasing, of course, the proportions of the fibrin and of the globules in that fluid. Now, it is obvious that this first effect of saline purgation is *not* debilitating. But what is the next consequence? To repair the loss of serum, absorption from the aqueous contents of the stomach and bowels, and also probably of the aqueous vapour in the atmosphere through the lungs, rapidly and actively begins, and in a very short time the vascular system is re-distended with a serum of a much thinner and less stimulant quality than that of which it was lately drained. The nervous system, which, in regard to the vigour of its manifestations, is powerfully influenced by the quality of the vascular fluids, feels and betrays the sedative effect of this admixture; hence the rigors, neuralgia, and other symptoms, which precede, accompany, and constitute rheumatism.

With regard to certain forms of cardiac disease, I have had equally ample proofs that purgatives used without the greatest circumspection, and more especially that saline purgatives are eminently prejudicial.* In dilatation of any of the cavities this effect of purgatives is particularly observable. In this form of heart affection, whatever weakens the nervous and muscular vigour of the cardiac parietes, must increase the embarrassment caused by the dilatation, even so far as to bring on palpitation or irregular contraction of the walls of the affected cavity or cavities, an effect which we have often witnessed, and which I have repeatedly had occasion to remark in the cases of two patients now under my care.

Thus, a patient of mine who labours under structural disease of the heart finds the symptoms to be invariably aggravated by the use of tea, sloppy diet, or purgatives; while he derives sensible benefit from a *moderately* generous diet, from a moderate use of wine, beer or brandy, and from *constipation*.

Of the danger attending the use of saline purgatives in threatening or incipient phthisis, more especially in the case of strumous subjects, I presume every practitioner of any experience is satisfied. It would not be difficult to theorise plausibly as to the *rationale* of this effect of purgatives in the particular affection referred to, but as doing so would lead me into some details which would occupy greater time and space than can now be allotted to them, I shall defer entering on it for the present.

I shall, in concluding this notice, make

* I have elsewhere pointed out a singular affection, closely simulating disease of the spleen and heart, which is caused by purgatives and removed by tonics. (Derangements, Primary and Reflex, of the Digestive Organs, p. 264.)

mention of two cases which, though not connected with the subject of constipation, are of some practical interest and importance. The first is that of a man, about forty years of age, who, during several years, has been subject to attacks of epilepsy every six weeks or two months. I began by giving this man half-grain doses of nitrate of silver three times daily, which I gradually increased to two grains (thrice a day), carefully regulating, at the same time, his bowels, his diet, and his habits. In the course of a very few weeks, a remarkable improvement took place. The epileptic paroxysms were notably reduced in severity and the intervals between them increased. So sensible was this patient of the vast benefit which he derived from the nitrate of silver, and of the power which it possessed over his disease, that he was perfectly willing to run the risk of the discoloration apt to ensue from its persistent use, in consideration of the advantage which he felt was accruing from it. However, I next put him on a course of the ammonio-sulphate of copper, and lastly, of the oxide of zinc. The result of this treatment is that during nine or ten months he has not had a paroxysm. To those accustomed to treat this most intractable and discouraging affection the above fact conveys, in itself, a sufficient commentary.

The second case was that of a truly formidable obstruction of the bowels, attended with the grave symptoms of stercoraceous vomiting, of which Mr. Doughty, of Knightsbridge (in answer to a request from me that he would furnish me with the details and dates of the treatment), gives an account in following the letter:—

"4, William-street, Knightsbridge,
"Feb. 10, 1843.

"Dear Sir,—I think you saw the daughter of Mrs. ——, seven or eight days after my first attendance, during which time I gave her repeated doses of calomel and black draught. * * * * I saw her on the Friday: the calomel was retained on the stomach, but the draught was vomited; she complained of pain in the head and nausea. I ordered four grains of calomel to be taken that evening, with the senna draught in the morning, which I relied on operating; consequently, I did not see her till Monday morning, when I learned that the draught had been vomited, and that there had been no action of the bowels; she slept a good deal; the pulse was pretty regular; but she still complained of vertigo. I repeated the calomel, combined with cathartic extract and an aperient draught. When I saw her on Tuesday the latter had been vomited and the sickness increased, with hæmatemesis. Saline mixture, with small doses of hydrocyanic acid, was then given to allay the nausea. I saw her on Wednesday; she told me the saline mixture made her more sick. Small doses of calomel were then given every three or four hours, with a strong aperient dose occasionally. It was after this that the stercoraceous vomiting commenced; and you saw the case, and ordered the turpentine enema with castor oil, and, I think, calomel in small doses. On the following day you prescribed six ounces of mercury and the tobacco enema, sixteen ounces of which, Pharmacopœia strength, I administered. This produced syncope and cold sweat. I think on the following day eight ounces more mercury were given, and the tobacco-smoke, to the extent of five or six whiffs, was administered, and in the course of the day croton oil, per mouth and rectum; and on the following, or second day after, the tobacco-smoke was again used, and after it an assafœtida enema was given. After the latter we began to have copious stools, and the whole of the mercury passed.

"In the evacuations there was a substance very like coffee-grounds, and on questioning Mrs. ——, I learned that her daughter was in the habit of chewing whole coffee very freely. Is it not possible that this was the primary cause of this obstinate case of obstruction. I am, dear Sir, your's truly, "E. D. Doughty."

I may add to the above detail, that immediately after I saw the case I ordered calumba and opium, to arrest the vomiting, an object always to be attained, if possible, in such cases. I also directed the attendants of the patient, after the quicksilver was taken, frequently to raise her up in bed (she was too feeble to raise herself), and to alter her position from one side to the other, from the back to the belly, and *vice versa*, with the view of favouring the gravitation of the quicksilver to the lower bowels. My object in ordering the tobacco infusion and smoke enemata was to favour the reduction of any obscure hernia or muscular spasm of the bowel, supposing any such to exist. The issue of the case was extremely satisfactory, and to me an agreeable disappointment; for certainly, from the whole aspect of the symptoms, even from the moment I saw the patient, I despaired of the poor girl's life. I am, Sir, your obedient servant,

Robert Dick, M.D.
Upper John-street, Golden-square,
March 14, 1843.

The Rage for Invention.—Velpeau and Blandin have both deprecated the present mania for the invention of new surgical instruments in France, as if surgery were not already overladen with mechanical contrivance. Velpeau attributes this mania, which prevails mostly among young surgeons, to the ignorance of these youths as to what has been done before them; and he judiciously observes, that "it would be much better for them to study the capacities of the surgical instruments that at present exist than to invent others of which there is no need."

GENERAL ABSTRACT
OF THE
VIEWS OF CORRESPONDENTS OF "THE LANCET"
RESPECTING THE
FUNCTION OF THE SPLEEN.

(Concluded from p. 821.)

Mr. Eagle, in an elaborate paper, proceeds to reply to the criticisms elicited by his communication of September the 3rd, ult. (1841-2, vol. ii., p. 783), in "the lump," as is quizzically remarked by our correspondent, "Milesius" (p. 130). His first response is to "Amicus Veritatis," who had first objected "that Mr. Eagle had thrown overboard the idea that the lymphatics were the excretory ducts of that organ, and had too hastily concluded that the product of the spleen passed into the circulation by the vein." Mr. Eagle avers that his reasoning on that point was sound and logical.

"'Amicus Veritatis' has said, 'If, as Mr. E. supposes, the product of the spleen be the colouring matter of the blood, we ought to find that material in greater quantity in the blood of the splenic and portal veins than in that, perhaps, of any other part of the venous circulation. But Mr. E. quotes the very authorities which demolish his own hypothesis. M. Schultz states, 'The blood of the vena portæ is, in general, more dark than venous blood. It is not reddened by solutions of neutral salts, nor by the action of atmospherical air, nor by the action of oxygen gas.' Now, the darkness of the blood in the vena portæ arises principally from the slowness of the circulation, especially in the intervals of digestion, and even arterial blood becomes dark from the same cause; and, as to the oxygenation of the portal blood, it is distinctly stated in my paper that this blood is rendered susceptible of aeration in the liver, so that in fact the colouring matter is not perfected until it is aerated in the lump, and hence the tail of my proposition, 'that the product of the spleen is the colouring matter of the blood *in its primitive condition*.' And I may here add, from examination of animals after excision of the spleen, I have a suspicion that another part of the animal organism assists the spleen in this office, and in a *degree* only compensates for its removal. Let the quantity of pigment, then, be ever so great, the dark colour of the portal blood, and its insusceptibility to aeration and oxygenation, would still obtain.

"I thus accuse 'Amicus Veritatis' of faulty logic, and at the same time of misrepresentation in this, that he holds back the necessary explanation of the non-aeration of the portal blood, which runs thus in my paper of September 3rd:—'The chylous and mesenteric blood joining the splenic product becomes dark red in the porta, to prepare the bile in the liver, where it is also rendered susceptible of aeration.' Thirdly, he quotes Dr. Clanny's remarks on the *general* (red blood) as if they were made against the *peculiar* (the portal blood), which is to be guilty of 'argument by illusion or false show.' 'Amicus Veritatis' is pleased to characterise my *facts* as meagre and insufficient, and my *reasoning* as jumping at conclusions. I rather question his competency to adjudicate the matter, but surely it is better to have even a jump at the *truth* than lazily sit still in the stink of old, diseased, and putrid *error*.

"The writer observes, by way of conclusion, that he has 'no wish to beg the question of Mr. Eagle, but would meet him on his own ground, with the query—if the spleen is to be considered a gland, where is its peculiar duct?' I answer distinctly, the splenic canal is the peculiar duct, and I invite 'Amicus Veritatis,' or any anatomist in London or elsewhere, to show either that the splenic vein of an ox resembles any other vein in the body, or that in its special anatomy it does not closely resemble other ducts, as the duct of the pancreas, for example. Here is a specific challenge, without either rigmarole or supererogation.'"

Mr. Eagle next attacks the allegation of the vital contractility of the spleen, as put forth by Mr. Jackson.

"Mr. Jackson's hypothesis, that the spleen is an assistant-circulatory organ, falls to the ground, if it can be shown that this organ possesses no vital contractility whatever, no propulsive power. Now Mr. Dalrymple has demonstrated (Lancet, Dec. 11, 1841,) that fibrous bands exist in the placenta, but who has demonstrated, or ever will demonstrate, fibrous bands in the spleen?

"The placenta is fibrous and actively contractile, the spleen is non-fibrous, and only passively (if at all) contractile; and this fact I have witnessed twenty times on opening the abdomen of the rabbit, both before and after excision of the spleen. So far as my experiments have gone (and I endeavour to see with my *eyes* and not with my *prejudices*), neither the splenic or the portal vein possesses any vital contractility whatever; indeed, that they should seems to me a physical impossibility. 'The radicles of the spleno-hepatic vein,' says Mr. Jackson, 'possess the property of vital contractility, *as is proved by the spleen varying in size at different times in the same animal*.' Is this what Mr. Jackson calls a *demonstration*? That the spleen varies in size at different times is *a fact* of which I have had ocular demonstration; but the *inference* of its vital contractility is loose and illogical; for, unless the subject of disease, the spleen is, I believe, always larger during digestion, when the secretion of its product goes on most actively. The same may, I think, be

said of the varying size of the pancreas, and even the liver.

"These are some of the reasons on which I form the opinion that Mr. Jackson's view of the function of the spleen is a mere barren hypothesis, leading to no improvement in medicine or surgery. It leads to nothing, explains nothing. Does it point to the periodicity of ague and the coincident enlargement of the spleen? Does it take cognizance of the altered coagulability of the blood constantly following excision of the spleen? Does it explain the fattening of animals under such circumstances? Does it tell of the condition of the porta in cases of death from starvation? No, it leads to nothing, it explains nothing. It tells of an organ possessing high vital contractility almost *without nerves* (Haller)—of an assistant-circulatory organ without *power*—of an organ for the specific purpose of carrying on the hepato-portal circulation, and yet (if getting fat and hearty are any proofs of a good circulation) this circulation goes on much better without it, even in a weak animal!!! In the name of good sense, then, let us have some better reason for Mr. Jackson's belief than the wretched logic 'because the heart *cannot* propel the hepato-portal blood, *ergo*, the spleen *must*;' for that is to argue from ignorance rather than from knowledge.

"And what says 'Amicus Veritatis,' for I had almost forgotten his hypotheses, and might safely have left them to *demolish* each other. He first fully argues with Mr. Jackson that the spleen is an assistant-circulatory organ, yet has never seen any muscular fibres to endow it with actively propulsive powers; secondly, he believes that its main office is to fit the portal fluid for circulation in the liver, by abstracting from the blood supplied to it part of the watery and albuminous particles, yet does not even show that the fluid *is less watery*; he next believes that its office is *quasi-glandular*, yet is horror-struck at the mention of a *duct*. A little further on, and lo! another change. He now believes its function is not *single* but *double*, its *products* of two kinds, and carried away by two sets of *vessels*; not *ducts*,—of course not,—but *vessels!!*"

In our simplicity we had thought that so grave and mysterious a subject as the present scarcely admitted of poetic licence. Mr. Eagle, however, undeceives us, for, entertaining the opinion of Dr. Collier, "that most human inventions have been originally anticipated by the poets, and that the philosophers followed in their wake" (LANCET, 1841-2, vol. ii., p. 883), he gives us a number of extracts from Pope and other poets, to prove that disease of the spleen is productive of melancholia. We regret much that we are obliged to omit these, for they are very "pat to the purpose" of Mr. Eagle, and their insertion here would have amply proved the truth of Mr. Jackson's quotation, "*splen ridere facit.*"

Mr. Eagle returns to the asseveration that the so-called splenic vein is, in fact, a sinus and a duct, resting this statement on the special anatomy of the canal, its coats, &c.; on a comparison of it with the hepatic and other ducts, on the one hand, and the pancreatic and other ducts, on the contrary; on the relative position of the artery, which in the ox lies "frequently, if not commonly, within the circumference of the canal, separated from it truly by its proper cellular sheath;" and on the many vascular mamillary points which break the continuity of surface in the canal, and off which what he considers the *pigment* of the blood may be wiped. Respecting the pigment, then, Mr. Eagle says:—

"The function of secreting this product I assign to the spleen. To the pancreas, I think, from experiment, I shall assign the function of secreting the oily matter of the blood, and thus the mixing of the alkali, the bile, with this oil, is the principal cause of the milky nature of the chyle and of the chylous serum, almost independent of the nature of the food taken into the stomach. * * * There is a very remarkable case of melanosis related in THE LANCET of July last (1841-2), which strengthens unavoidably, as I think, my view of the 'anatomy and physiology' of the spleen, for on examination after death, coincident with a pale appearance of the body generally, the spleen was found so changed in structure, as to resemble pigment of peroxide of iron. Now where, I ask of 'Amicus Veritatis' and of Mr. Jackson, was this pigment formed but in the spleen itself? Which of the three hypotheses now agitated in THE LANCET offers the best solution of this case?"

Towards the termination of his paper, Mr. Eagle replies at length to "Milesius," for whose letter see ante, p. 130. Very few *fresh* facts are brought forth in this reply, and without detriment to Mr. Eagle's views, readers may safely be left to form a judgment on their merits, or those of "Milesius'" objections, from a perusal of their original papers. Mr. Eagle deduces from his experimental excisions of the spleen in animals, not only that the removal of that organ is directly productive of fattening, but that it prevents the deposition of tubercles; and, in support of this opinion, he quotes a statement by Dr. Carswell, "that it is very rare that tuberculous matter can be detected in the blood contained within its proper vessels, but that it is frequently met with in this fluid in the vessels of the spleen." In conclusion, Mr. Eagle congratulates himself that his researches on the spleen have led him, first,—to the proposition of an operation which, when fully and fairly developed, will, as he believes, soon become a standard operation in surgery; and, secondly, to the ob-

servation of the altered coagulability of the blood after excision of the spleen, which has led him to transfuse into the blood a medicinal agent, with the view of at once (in the language of Hewson) *so increasing the coagulability of the blood* that no acute inflammation, local or general, can possibly proceed.

"The first operation has been already *in part* laid before the readers of THE LANCET; and it is to be followed by the second, as time, cases, and observations occur; not, however, until its effects admit of such *demonstration* as to place it beyond the reach of a peddling controversy."

Mr. Jackson begins a letter, dated Dec. 5, 1842, by thus addressing the editor:—

"According to Mr. Robert Stevens, it diverts arterial blood into the portal vein! This notion of Mr. Stevens is exactly the converse of the last. It asserts the very opposite of the 'highly carbonised pabulum,' namely, a 'highly oxygenised pabulum.' 'Amicus Veritatis' says that the liver must be supplied with a *very venous* blood; Mr. Stevens that the portal blood is already *too venous*, or would be, if it were not mended by the addition of a certain quantity of arterial blood."

Mr. Jackson, then, considering "Amicus Veritatis" as demolished, by no answer having appeared to his former objections (ante, p. 88), directs his cannonade against Mr. Stevens.

"The spleen of a man whose liver weighs three pounds, averages about *eight* ounces. The spleen of a pig, the liver of which weighs three pounds, is only *four* ounces. In the former, therefore, the spleen is one-sixth the weight of the liver; in the latter one-twelfth. Now, as the spleen of the pig is only half the size of the spleen in the human subject, the splenic artery of the pig will be only half the size of the splenic artery of the man; and the quantity of oxygenised blood (it *is* an imperfectly carbonised blood) diverted through the medium of the spleen into the portal vein of the pig, will be only half the quantity of oxygenised blood diverted through the medium of the spleen into the portal vein in man. Can Mr. Stevens tell why the portal blood of a pig requires only half as much oxygenised or splenic blood to be mixed with it as the portal blood of a man? Why, the pig's liver does not require so highly oxygenised a pabulum as the man's liver? or why a pig's spleen should be only *half* the size of the man's spleen, *when the liver of each weighs the same?* Now, where is the oxygenised pabulum?

"*To learn the use of the spleen*, a man must sit down quietly, and ask himself how the blood gets through the liver both before birth and after. He must apply his mind to the fœtal as well as to the adult circulation, or perhaps he may stumble on Mr. Shaw's mare's-nest. Mr. Shaw, in the 'Medical Gazette' for July 15, 1842, tells us that respiration pumps the blood through the liver. I never replied to this notion, for I thought by so doing I might prevent Mr. Shaw showing us, in a subsequent number, how respiration pumps the blood through the liver *before* birth as well as *after*. A man should be cautious how he blunders in the face of demonstration.

"The circulation ought properly to be divided into *three* systems: a portal, a pulmonary, and a general; instead of *two*, a pulmonic and systemic. The disagreeable conviction might, indeed, come home to some of your readers, that they had only previously understood *two-thirds* of the *circulation*, and these imperfectly; and that their ignorance of the remaining third is the cause of their knowing nothing of such organs as the spleen and the placenta, *cum quibusdam aliis*, concerning which I now say nothing.

"Again: if it be true that all the fluids taken into the stomach pass through the veins of that viscus into the splenic vein (the veins of the stomach and also of the duodenum terminate in the splenic vein); and if, therefore, it be *not* true that the fluids taken into the stomach pass through the absorbents, and climb up the thoracic duct to get into the veins (and the opinion that they are is so hugely preposterous that it is wonderful it should ever have been entertained); and, again, if it be true that there are men who drink two, three, and even four or five gallons of fluid per diem,—then there can be no doubt whatever that much more diluted venous blood enters the heart by the hepatic veins than there is arterial blood supplied to the digestive organs by the cœliac and mesenteric arteries.*

"If it be impossible that the left ventricle, by propelling *one ounce* of blood through the cœliac and mesenteric arteries, can effect the propulsion of *ten drachms* through the portal plexuses and hepatic veins into the right auricle (and it is manifestly as impossible as for a force to overcome a resistance greater than itself); then it is absolutely certain the *vis-à-tergo* of the left ventricle does not propel the portal blood through the liver into the heart.

But we know the blood *is* propelled through the liver into the heart both before death and after; and if it be in neither case the heart which effects that propulsion it is clear some other organ must; unless, indeed, the blood-globules are wheels which can move of themselves and carry along the fluid in which they are contained. In two or three communications to your Journal I have en-

* Only a few days ago a physician assured me that he is acquainted with a person who drinks, on an average, five gallons daily. I understood that it was chiefly beer.

deavoured to show that the agent by which the blood is propelled through the liver before birth is the *placenta*, and after birth the *spleen;* and that such office is the chief, nay the sole use of both organs; for I am at a loss to understand why any *one* organ should perform *two* functions. I have, moreover, endeavoured to show that the spleen consists essentially as well as chiefly of the radicles of the spleno-hepatic vein, and the placenta of the radicles of the umbilical vein; and that the former ought to be regarded as a permanent placenta, and the latter as a temporary spleen.*

"I must again remind your readers that the branches of the spleno-hepatic vein are surrounded by a sheath of loose cellular membrane, a prolongation of Glisson's capsule, and that *the spleen* (or radicles of the spleno-hepatic vein), *is known to vary at different times;* which two facts indubitably prove the whole of the spleno-hepatic vein to possess the property of vital contractility. I must also repeat that the much larger size of the spleen in man than in the quadruped is manifestly owing to the different direction of the portal vessels; that is, to the venous current through the liver of the former being vertical, and through the latter horizontal; and from this one fact I infer that not only is the use of the spleen such as I have before pointed out, but also *that it can have no other use.* This may to some seem a bold inference; but the fact is strong enough to bear it. Nay, it proves more; for, coupled with the close resemblances which exist between the spleen and placenta, it leads irresistibly to the conclusion that it is no part of the use of the placenta to alter either the blood supplied to it by the mother or returned to it from the fœtus.

"The spleen and the placenta, therefore, are to be regarded as *assistant-circulatory organs*, and assistant-circulatory organs *simply;* for again I affirm that there is no reason whatever for believing that either one or the other is for the purpose of sanguification, or secretion, or to effect a 'quasi-glandular' change upon the blood, any more than there is for believing the heart is for sanguification, or secretion, or to effect some imaginary change upon the blood. The placenta before, and the spleen after birth, propels the blood through the liver; the right ventricle propels it through the lungs, and the left ventricle through the general system, *but not through the portal system*, which is a *distinct* system, just as the pulmonary is a distinct system of circulation; and it is, as I have already shown, as physically impossible for the left ventricle to propel the portal blood through the liver, as it would be for the spleen or spleno-hepatic vein to propel it through the lungs, or the right ventricle to propel it over the general system. *The circulation is, then, obviously composed of* THREE *systems, and not of* TWO, *as at present supposed.* There is a *portal* system, a *pulmonary* system, and a *general* system; and those physiologists take an erroneous and confined view of the circulation who imagine the portal system to be a part of, or a dependency of the general system. Seeing, then, that there are *three* capillary systems through which the blood is propelled, is it reasonable, knowing that the two latter have distinct agents for the propulsion of the blood, to suppose that the former is not provided with its appropriate and peculiar agent for that express purpose, but that it is dependant on, or indebted to, one of the latter—the left ventricle? And are we for ever to be Malpighi-corpusculizing and marvelling as to what possible use there can be for two such organs as the spleen and the placenta? If there be, as Abernethy affirmed, 'such a thing as common sense,' surely our creed of two hundred years' standing, that there is only one organ concerned in the circulation of the blood, has endured long enough. It was miraculous that the use of the heart eluded discovery until two hundred years ago; but it is the miracle of all miracles that two centuries after the discovery that the blood does circulate, men should still be at a loss to understand the purposes fulfilled by organs of such magnitude as the spleen and the placenta; and entertain not the smallest doubt that the *heart propels the blood through the liver, not only after birth, but before!* For of that which we have been taught, and which we have learned to believe true, a suspicion that it may possibly be erroneous seldom crosses our minds; and even then boldly to question its truth, is deemed a sort of infidelity of which few of us dare to be guilty, and which we also regard as almost unpardonable in others."

It may have been strange enough that the use of the heart eluded discovery until the

* "In an article by Mr. Dalrymple, published in the last volume of the 'Medico-Chirurgical Transactions,' that gentleman states that 'the whole mass of the placenta is made up of the innumerable ramifications of the umbilical arteries;' which is manifestly as erroneous as it would be to say that the umbilical cord consists exclusively of the two umbilical arteries. Mr. Dalrymple shortly afterwards says the umbilical 'vein and its branches (Mr. D. of course means the roots, for the branches are in the fœtus) are greatly larger than the arteries and their subdivisions, but less numerous." I beg to direct the attention of your readers to Mr. Dalrymple's paper on 'The Structure and Functions of the Human Placenta;' and more particularly to the concluding physiological remarks, which I think valuable inasmuch as they afford a pretty fair specimen of the sort of service which microscopical anatomy is capable of rendering to physiology."

time of Harvey; but it is miraculous that, that discovery once established, the true purpose of organs of such magnitude as the spleen and placenta should have remained misunderstood, and apparently little cared about, for two centuries afterwards.

Our correspondent, "Amicus Veritatis," says, in a letter dated Nov. 2, 1842:—

"Severe illness has hitherto prevented my answering the objections to my view of the function of the spleen which have been started by Mr. Jackson in THE LANCET of Oct. 15th last, but lest I should seem to shew the 'white feather,' I hasten now to answer them briefly, and for the most part *seriatim*. At his outset (ante, p. 88) Mr. Jackson attributes to my communication in THE LANCET of Sep. 24 (1841-2, vol. ii. p. 878), a motive certainly not warranted by its drift, when he says that its object was 'to shew that he was wrong in regarding the spleen as an assistant-circulatory organ *simply*, and the placenta as a temporary spleen *only*.' It is evident that his notion, as manifested by this remark, is that the spleen could only be an assistant-circulatory organ in so far as it actually *propels* the blood, and the placenta a temporary spleen only in so far as it exerts an analogous action. But is no viscus to be considered as aiding in the function of circulation but such as have an absolute propulsive power? May no gland or membrane be invested with the title of assistant-circulatory organ, which alters the blood and renders it more fit to pursue its subsequent course? It is, on the contrary, in the latter respect, chiefly, that I regard the spleen as facilitating the circulation; and by a parity of reasoning, I consider the lungs, liver, kidneys, &c., in so far as they affect the circulating fluid, and withdraw from it or add to it some of its materials, as active assistants in the circulatory process.

"The spleen is clearly no gland, since it has no specific duct, and gives origin to no specific product, as do the liver, kidney, or pancreas. But I believe that it still exerts a certain action upon the blood supplied to it, and I will beg for space to recapitulate here what I consider this action to be. *Selon moi*, then, the chief use of the spleen is *to fit the portal blood for circulation in the liver by abstracting from the blood supplied to itself the greater part of its lymph, so as to present to the liver a highly carbonised fluid, and one especially fitted for the elaboration of the bile*, which, as is well known, is in all normal cases secreted from the portal blood.

This action of the spleen I term *quasi-glandular*,* seeing that the change impressed by that organ on the portal blood is somewhat analogous to the results of glandular action; while, the spleen being no gland, it cannot strictly be called a glandular change. Another viscus, the lung, effects an important change on the blood, to which, for the same reason, viz., that the organ is not a true gland, I would also apply the term of a *quasi-glandular* change. And, indeed, were I disposed to class the spleen in the same category with any other accessory organ of the circulation, it would be with the lung, to which it has at least a physiological resemblance, and of which organ the placenta has been uniformly admitted to be the representative during fœtal life.

"Now, as to the placenta, most of us have hitherto been taught to look upon that organ as one in which the blood of the fœtus undergoes some change, by which change (if not rendered in a degree similar to the arterial blood of the mother, by the absorption of oxygen therefrom, or receiving an alteration analogous to that effected on the blood of the chick *in ovo* by the allantois, the evident analogue of the placenta in mammalia) it is assuredly rendered fitter for circulation in the fœtus; or, if Mr. Jackson will rather have it so, 'through the umbilical vein and the fœtal liver into the fœtal heart.' How then, Sir, have we to unlearn all that distinguished authorities † have led us to believe (for actual *evidence* there is none)? Have we to learn to admit that the blood of the fœtus (so unlike to what occurs in after life) circulates through the body perpetually, without requiring any change, anything similar to the decarbonisation or arterialisation of after life?—a process which can by no means be supposed to take place within the body of the fœtus, and which is certainly effected in the chick through the medium of the allantois. (See the trans. of Wagner's Elem. of Physiology, p. 130, &c.) Notwithstanding what Mr. Jackson says, I must, for the present, at least, be permitted to continue my belief,—firstly, that the main function of the placenta is rather to impress a change on the fœtal blood than to propel that fluid into the fœtus; secondly, that the ventricles of the fœtal heart are quite adequate to the task of propelling the blood through the *arterial* system of the fœtus; and thirdly, that whatever be the *primum mobile* of the *venous* circulation, the auricles

* "Mr. Jackson, who professes to require information as to the meaning of both words, will find *pabulum*, in Ainsworth's Latin Dictionary, to signify 'aliment,' food, forage, and in a similar sense it was used by me. But I will now, as far as possible, keep to plain English."

† To adduce two authorities only, the reader is referred to Dr. Davis's work on Obstetric Medicine, vol. ii. pp. 837, 842; and Wagner's Elements of Physiology, translated by Dr. Willis, part i., ch. 1, 2. Dr. Rigby, in his comprehensive and admirable System of Midwifery, in the Library of Medicine, corroborates the views of these authors.

of the heart can act after the manner of a *pump* as well during fœtal as post-fœtal life.

"I have never denied, nor do I now deny, that the spleen and placenta may exert a contractile agency; nor even that their contractile force may be in some (though still a minor) degree their function. But I strongly protest against confounding vital contractility with an active propulsive power, as Mr. Jackson has done (ante, p. 89). An actively propulsive power can only belong to an organ composed of muscular fibres. Who, for instance, would confound the contractility of the corpora cavernosa penis with the action of the ventricles of the heart—two excellent specimens for displaying the difference between merely elastic and actively propulsive force? I see yet no reason to abandon my former proposition (which has, I think, been but feebly met by Mr. Jackson, ante, p. 90), that if the exertion of a contractile power were the chief, or, as Mr. Jackson asserts (p. 88), 'the simple and sole purpose' of the spleen, we should naturally expect to find the organ so placed as to be able to exert that contractile power to the greatest purpose in the animal economy. What is the fact? Why, that a large proportion (perhaps a half) of the blood entering the vena portæ never passes at all through the spleen, which, consequentiy, can exercise its contractile force on only a portion of the blood which passes through the liver, having little, if any, control over the rest. The intended purpose of Nature would, then, seem to have been only half-effected. Nature seldom does her work in so clumsy a manner!

"In a previous letter Mr. Jackson regarded the spleen, by its contractile force, as the *regulator* of the portal circulation; but in that of Oct. 1, the splenic vein, portal trunk, &c. (veins being structures to which, unlike arteries, vital contractility has not been usually accorded), and even the capsule of Glisson (?), have been associated by him with the spleen in its responsibilities. Having thus shifted his ground, upon the presumption of the active aid of these subordinate structures, Mr. Jackson (by a process of reasoning which I pretend not to understand, but to which I refer the reader, ante, p. 90) goes on to account for the portal circulation still proceeding, notwithstanding that the spleen may have been extirpated. Truly, it would be wonderful did we not find the moderns almost apt to return to the opinion of old Erasistratus, that the spleen is an useless organ, which Nature might have spared herself the pains to construct, when they find one physiologist declaring that its functions may be carried on by accessory and subordinate structures, and another attempting to show that we are sometimes better without a spleen than with one, and proposing, at such times, an operation little less formidable than the tying of the common iliac artery, not to say the aorta!

"Other points in Mr. Jackson's reply would demand as much comment as the foregoing; but I have already perilled my promise of brevity. 'Brief let me be,' then, with the rest. Mr. Jackson very gratuitously denies that any reason at all can be offered why the spleen should effect any peculiar change on the blood supplied to it (p. 88). It is difficult, in the present incomplete state of our knowledge respecting the animal fluids, to demonstrate that it does so. But the researches of the German chymists (Schultz and others) are in general worthy of credit, and from these, from taking note of the intimate connection of the spleen with the rest of the chylopoietic viscera, and particularly from remarking the *great number of lymphatics* that issue from the spleen, I have been led to form my view of the function of that organ. And it appears to me that this is a far more philosophical mode of drawing a conclusion than Mr. Jackson has adopted, who considers that the placenta propels the blood through the umbilical vein, &c., *because nothing else can* (p. 89); or assigns to the capsule of Glisson a contractile power as its function, apparently *because no person has assigned to it a better one.*

"Mr. Jackson's observation that 'the portal blood is, and must be, a fluid varying in composition and quality, almost as much as the contents of the stomach,' seems intended to cast discredit upon the opinion that that blood can be affected at all in its constitution by the action of the spleen. No doubt, *all* the fluids of the body present at different times minor aberrations from their normal constitution, consequent on change of diet, or other causes; but their essential constituents are commonly the same. Under all ordinary circumstances, the blood, the chyle, the lymph, are composed of their own peculiar elements, unchanged, for the most part, by slight differences in the diet or habits of the individual; and, at any rate, a material change in their chemical constitution is always accompanied with grave disease. We may safely presume, then, that the portal fluid, among the rest, in general preserves its proper characteristics and composition, and that it invariably differs in its character from the venous blood at large (as found to be the case by Schultz), as well as from arterial blood.

"I must beg leave altogether to decline setting up a reason why the spleen of a pig is, as compared with the liver, of only half the relative weight of the spleen in the human subject. It may be, indeed, as Mr. Jackson surmises, that the pig, from its position on all-fours, does not demand an organ exerting so great an amount of elastic force as does man; and it may be so without the admission involving a necessity of acknowledging that its contractile power is the chief

function of the spleen. But I freely confess my ignorance of the reason of the above difference; and I willingly leave to those more closely engaged in the practical pursuits of comparative anatomy the task and the glory, together with the better opportunity, of framing new hypotheses to meet the occasion."

"Amicus Veritatis" excuses himself from the further contest on this subject in consequence chiefly of the time absorbed by it. He says, "I started my propositions; the reply has been made; so has the rejoinder; and I am now fairly entitled to quit the field. In withdrawing from the discussion my chief regret is lest I may thereby deprive your other readers and myself of opportunities for enjoying Mr. Jackson's jocularity, which occasionally breaks forth in a most mirthful and entertaining manner, as, for instance, in THE LANCET for Sept. 24th last, pp. 881-2. And I sincerely hope that when the discussion on the spleen has terminated some one may hit upon another subject for a controversy in which Mr. Jackson will join."

And so say we.

The remarks of our correspondents, Mr. Ross and Mr. Way, on the subject of the spleen (pp. 679, 823), are too recent to be lost from the minds of our readers, and therefore to demand recapitulation. It may be noticed, however, that in so far as he attributes an important function to the lymphatics of the spleen, Mr. Ross in some degree supports the views of "Amicus Veritatis;" while the observations he records fully bear out the statements of Mr. Eagle respecting the fattening of animals after excision of the spleen, and his subsequent commentary is explanatory of the steps by which this result occurs.

We have thus laid before our readers as succinct a view as possible of the opinions of our several correspondents on the subject of the spleen; they can choose for themselves from among the theories already propounded, or invent new ones of their own. But we would suggest to future controversialists that their communications should be sufficiently brief and pointed to save them from the mutilation which we have been more or less compelled to practise in the present instance. An admirable motto prefixed to a late communication by Mr. Jackson, "Let there be a fair discussion. Remember we fight only with foils, and the point of mine shall be covered with velvet,"—suggests to us one more remark. Let not the controversy on the *spleen* become *splenetic*. Truth was never yet elicited by passion.

CURE OF ABSCESS OF THE TOOTH.

To the Editor of THE LANCET.

SIR,—In THE LANCET of Dec. 3, 1842, you published an article upon "intra-dental abscess," with remarks on the "use of a vent in the stopping of decayed teeth." As I am opinion that you would wish to correct error on the subject, I have thought that a communication from this side of the Atlantic would not prove unacceptable. Your valuable publication being now republished here by one of our most enterprising firms, that of Messrs. Wilson and Co., many persons in America have the advantage of perusing its columns, and thus the above article came under my notice, when perceiving at once the palpable mistake that it contains, I determined to explain the unsoundness of the theory, and the uselessness of putting in practice the propositions therein named.

Mr. Saunders (the author of the paper) says, that when the internal cavity of a tooth is suddenly exposed (the nerve and its membrane, of course, is to be understood) by the breaking down of the thin layer of carious bone which protected it, by "injudiciously" stopping it inflammation and suppuration speedily supervene. He afterwards proposes, in order to obviate the difficulty of intra-dental abscess, to plug up the cavity in the tooth, and drill a hole through the "stopping" so as to reach into the cavitas pulpæ, and thus form "a little canal" as an outlet for the pus secreted therein, as well as for the tooth's pent-up nervous irritability. The theory is ingenious, but the practice unfortunately does not preserve the tooth, nor will protect its owner from toothach.

Mr. Ghrimes, in THE LANCET of Dec. 17, 1842, satisfactorily proves the mechanical uselessness of the "little canal," but unfortunately offers, as an amendment to the method proposed by Mr. S., a proposition which is worse than useless, namely, that of drilling a hole from the neck of the tooth, through its substance, into the internal cavity, to answer as a substitute for the little "canal of Saunders." This also has the same objection, that it will not prevent or arrest the decaying of the tooth, nor prevent its aching, while, in addition, it would allow the decay to undermine the stopping, so that perchance the patient would one day discover that with his meal he had swallowed the crown of his tooth, leaving the "stump" in its place. It is dangerous to the tooth to meddle with its neck in any way. Ample proof of this fact is offered in the serious injury done to sound teeth, where clasps, wires, or ligatures are applied around thei necks to secure artificial teeth, the banefu practice of some dentists. The neck of the tooth, externally, is its most tender and vital part.

Mr. Saunders candidly admits that his practice does not effect a cure; nor did he attempt it, believing it to be, as he says, "impossible, at least in the present state of dental surgery," to arrest the suppurative process when once established "under such circumstances and in such a situation." With all due deference to Mr. Saunders's experience, and his opinions of "the present state of dental surgery," I beg to state that not only an arrest, but also a perfect cure, may and can be effected, and the tooth effectually stopped, and rendered useful for life. The following formula,

℞ *Ox. arsen. alb.*, 1-20th gr.;
Pulv. gall., ½ gr.;
Opii gr. ss;

made into a paste, and applied into the cavity of the tooth, and kept there either by a softened piece of wax, or a pledget of lint saturated with creosote, will entirely remove it, in one application. All that is necessary afterwards, for two or three days, is to apply a little dry sulphate of lime, introduced into the tooth, to absorb the little corruption left there. The tooth may then be sponged out with eau de Cologne, and filled or stopped, either with gold foil or a paste composed of marmora and silica. I have followed this practice for fourteen years, and with the exception of two cases within that time I have never failed to eradicate the disease, and effect a perfect and permanent cure. I remain, Sir, your most obedient servant,

A. C. Castle, M.D., Dentist.
New York, Feb. 25, 1843.

ON SOME OF THE

CAUSES WHICH DETERMINE THE MINOR PERIODS OF VITAL MOVEMENTS;

(Being a Third Contribution to Proleptics).

By Thomas Laycock, M.D., Physician to the Dispensary, York.

In my last paper I observed that the cycles in animal life I had demonstrated, were connected either with periodic agencies inherent in the system of the animal, or with agents acting externally to it, or else were dependent upon a combination of the two. I termed the first *esoteric*, the second *exoteric*, the third *endexoteric*. Such of the esoteric revolutions as I considered to be well established by experiment and observation, I described in detail. I now propose to consider the exoteric agencies recurring at minor periods, and the changes dependent upon them. In my previous papers I attempted to demonstrate the following propositions:—

1. That there is a general law of periodicity which regulates *all* the vital movements in *all* animals.

2. That the periods within which these movements take place admit of calculations approximatively exact.

3. That the fundamental unit,—the unit upon which these calculations should be based,—must for the present be considered as one day of twelve hours.

4. That the lesser periods are simple and compound multiples of this unit, in a numerical ratio analogous to that observed in chemical compounds.

5. That the fundamental unit of the greater periods is one week of seven days, each day being twelve hours; and that simple and compound multiples of this unit determine the length of these periods by the same ratio as multiples of the unit of twelve hours determine the lesser periods.

To prevent any controversy I would observe, that these propositions contain what I claim as my own. I lay no claim to the discovery of the idea of periodicity in vital function, nor of priority in the application of that idea; both these have long been common property. I think, however, that I am the first who has demonstrated the law which binds all periodic vital phenomena together,—the law which links the periods observed in the lowest annulose animals with those of man himself, the highest of the vertebrata.

The abstract idea of number is necessarily the foundation of all science. Being such, it is not a little remarkable that so little attention has been paid to its application to physiological researches. Its importance, no doubt, has been much overlooked; but it has also been neglected by inquirers from a fear lest their researches should present a visionary or mystical character. The learned vice-chancellor of the University of Cambridge has connected it more specifically with modern science than any one I know. His remarks may be found in his exposition of the law of symmetrical development. "We have an idea of symmetry," he observes, "and an axiom involved in this idea is, that in a symmetrical natural body, if there be a tendency to modify any member in any manner, there is a tendency to modify all the corresponding members in the same manner."* I quote this passage to refer to the recent papers of Dr. Budd and Mr. Paget on the symmetrical development of disease, and to the facts stated by those authors as remarkable illustrations of this axiom. I had, however, anticipated the application of the idea of symmetry to pathology in a recent publication of my own, in which I have shown how often disease is connected with an unsymmetrical and consequently abnormal development of structure and function

* Philosophy of the Inductive Sciences, vol. i., aphorism lxxx.

in the lateral halves and two ends of the nervous axis.* The general idea of number is, of course, involved in the idea of symmetry, but special numbers are specially involved. Thus, as Vice-chancellor Whewell observes, the influence of the numbers 3 and 5 is strongly marked in vegetables. The number 3 is the ground of the symmetry of monocotyledonous plants; the number 5 of the dicotyledonous. "This great numerical distinction, corresponding to a leading difference of physiological structure cannot but be considered as a highly curious fact in phytology."† The numbers 7, 9, 11, are rare numbers in botanical science; the number 5 influences animals also, as the radiata, but it is not produced in any inorganic bodies whatever.‡ I have already referred in a previous paper, to the remarkable prominence which the number 7 presents in the general law I have demonstrated, and have casually noticed the ancient physiological doctrines of Pythagoras and his pupils as being closely connected with this number. Vice-chancellor Whewell is led by his facts to a similar notice. I may say more on this point at a future opportunity. At present, however, I must confine myself to the number, as offering a clue to the discovery of the law of exoteric cyclical agency. From time immemorial the sun, moon, and planets, have been considered the great exoteric agencies, and the old science of astrology was probably, as I have already observed, a real science, developing their laws of action. The changes of the moon were, however, more particularly connected with the hebdomadal changes in the human system, because the periods of the two were observed to be of nearly equal length. This coincidence was, in fact, universally observed, and the resulting theory of lunar influence has modified the religion, rites, and domestic habits of mankind to an almost incredible extent. A theory prevalent amongst men in all ages and in all climes, and leading to such results, cannot be justly or safely neglected, however great the absurdity of gravely inquiring into its verity may appear. Whatever the facts may be, the literary history of the theory is very curious, and full of interest. There can now, however, be little doubt that vicissitudes in the atmosphere as well as in the ocean are dependent upon the gravitating power of the moon. Many observations have been made on the barometrical changes. Those of Horsburgh, of Humboldt, of Boussingault and Rivero, and particularly of our own countryman, Lieutenant-Colonel Sykes, have fully demonstrated the existence of regular tides in the earth's atmosphere which flow *independently of temperature*. But although these changes in the barometer may be independent of the sun, yet solar influence must not be omitted in an investigation of this kind; nor should we forget that the diurnal revolution of the earth on its axis may of itself influence the earth's magnetic condition.

Observations have been made on the variations of the hygrometer, barometer, thermometer, electrometer, and magnetic needle, at different periods of the day and year. In accordance with my established plan, I shall commence with the less general facts, namely, the diurnal variations; and I do this the more readily, because any general inferences deducible therefrom will apply to the greater variations observed at longer periods. I shall confine myself to observations made on the barometer, electrometer, and magnetic needle, as the moisture and temperature of the air vary much under circumstances purely local. A tabular view of the observations will best serve our purpose.

Table of the Diurnal Variations of the Barometer.

	Barometer at the Highest Point.	Barometer at the Lowest Point.
The Indian Seas (Horsburgh)	10 to 11, a.m.	4, a.m.
	10 to 11, p.m.	4, p.m.
Tropical South America (Humboldt).	9, a.m.	4, a.m.
	11, p.m.	4, p.m.
Do. do. Boussingault and Rivero	8 to 10, a.m.	4, a.m.
	11, p.m.	3 to 5, p.m.
The Deccan (Sykes).....	9 to 10, a.m.	4 to 5, p.m.
	10 to 11, p.m.	4 to 5, a.m.
Apartments of Royal Soc. (Lubbock) .	9, a.m.	3, p.m.
Do. do. (Hudson).....	10, a.m.	4½, a.m.
	11, p.m.	4, p.m.

Balfour made some observations at Calcutta, which are evidently correct as to the general fact, but as evidently incorrect with regard to the hours of variation. I have consequently omitted them.

* The Nervous Diseases of Women, 8vo. London, 1840. Part II., chap. viii.; on the Relations of Nervous Diseases to the Extremities of the Spinal Cord: chap. ix; Relations to the Lateral Halves of the Nervous System: see also part 1, chap. xi., sec. i., p. 195.

† Whewell, op. cit., vol. i., p. 427.

‡ Ibid. loco cit.

Table of the Variations of the Magnetic Needle.

	Maximum variation W.	Minimum variation W.
Barlow		6, a.m.
..	12 to 1, p.m.	.. 11, p.m.
Beaufoy	1½, p.m.	.. 8½, a.m.
Canton	1½, p.m.	.. 8½, p.m.
..		8½, a.m.
Fisher	12 to 1½, p.m.	.. 9 to 10, p.m.
..		9 to 10, a.m.

According to Canton's observations the hours varied according to the months, a fact to be noticed again. Mr. Christie deduced from tables given at length,* "that the maximum west variation occurred about 1½ o'clock, p.m., from which time the needle deviated to the east until nearly 9, p.m.; after which a small deviation to the west took place during the night, until an early hour next morning, from which time the deviation was easterly, until nearly 9, a.m., when it attained its maximum." There appeared to be a small maximum easterly deviation about 11, p.m., and an intermediate minimum east, about 4, a.m., previously to the absolute maximum east, about 9, a.m.

Table of the Variations in the Electric Tension of the Atmosphere. Re-arranged from a Table in Mr. Orton's very interesting Work on the Epidemic Cholera of India, p. 246.

	Minimum.	Maximum.
Read	6, a.m.	9, a.m.
..	4, p.m.	8, p.m.
Saussure	6, a.m.	8, p.m.
..	4, p.m.	9, a.m.
Schubler	5½, a.m.	9, a.m.
..	4, p.m.	

Tables of the hours at which sheet lightning (lightning without thunder) and thunder-storms occurred during the two years 1833-4, were published in the "Nova Acta Curiosorum," for 1835 (vol. xvii.), by Professor Schrön, of the Royal Observatory at Vienna. I have condensed these tables with the following results:—

Summer, or sheet lightning, was observed 55 times;
5 after midnight; but four of these were continuous with flashes observed before midnight.
1 about 4, a.m.
2 from 7 to 8, a.m.
47 from 8 to 12, p.m., but principally between 8½ and 10½, p.m.

Thunder-storms were seen 70 times. Of these 58, or nearly ⅘, occurred between noon and midnight; and 48, or nearly ⅔, between 8, p.m., and midnight.

It now remains to connect the diurnal esoperiodic changes noticed in my last paper with these meterological phenomena, and with the other facts I have quoted, which bear upon the subject. The most numerous and most exact observations have been made on the barometer, and if the table be examined it will be seen that the solar day is divided into two barometric days, of twelve hours each, each day presenting a second division into two equal parts, of about six hours each.

Table of the Meteoric and Physiological Events occurring at the Barometric Hours, during a Solar Day of Twenty-four Hours.

4 to 5 o'clock, a.m.

Barometer at its minimum height.
Minimum of electric tension, nearly.
Intermediate minimum variation east of magnetic needle.
Minimum of temperature.
Hour at which several flowers bloom.
Certain moths escape from the chrysalis.
Minimum consumption of oxygen.
Onset of cholera, epidemic diarrhœa, Egyptian ophthalmia, and quotidian ague.
Period of increased excitement in the insane commences.
Hour of alleviation of symptoms and of sleep in hectic and infantile fever.

4 to 5 o'clock, p.m.

Barometer at its minimum height.
Minimum of electric tension.
Minimum variation east of magnetic needle.
Certain moths escape from the chrysalis.
Termination of a paroxysm of quotidian ague.
Onset of a quartan ague.
Exacerbation of fevers; accession of hectic fever.
Period of increased excitement in the insane begins.

8 to 10 o'clock, a.m.

Barometer at its maximum height.
Maximum of electric tension.
Maximum variation east of magnetic needle.
Maximum excitability of the circulation.
Maximum of muscular power.
Period of increased excitement in the insane ends.

8 to 10 o'clock, p.m.

Barometer at its maximum height.
Maximum of electric tension.
Maximum variation east of magnetic needle.
Meteoric lightning and thunder-storms appear.
Certain insects escape from the chrysalis.
Consumption of oxygen at its minimum.
Minimum of muscular power.
Minimum excitability of the circulation.
Hour of natural sleep.
Period of increased excitement in the insane ends.
Paroxysm of a quartan ends.

* Theory of the Diurnal Variation of the Magnetic Needle. Trans. of Royal Society, for 1827, p. 308.)

What inferences can be safely deduced from these facts? 1. It is plain that the physiological and pathological day, as fixed at twelve hours, is of equal length with the meteorological day. 2. The hours indicating the four barometric divisions of the solar day are the hours from which the minor periods should be calculated. 3. There is a relation between the diurnal meteoric and vital changes. The nature of this relation is by no means evident; to have secured a guide to further observation is, however, a great point gained, and there can be no harm in speculating on the general facts.

1. What effect have barometric variations on animal life, and especially on the phenomena of epidemics? Huxham specially refers to the phenomena of intermittent fevers as being probably influenced by barometric variations through the varying pressure of the atmosphere on the veins.* More recently Sir D. Barry took up both the pathological and physiological views of Huxham, and in the same spirit observes, "It being now evident that the blood in the veins is placed under the influence of atmospheric pressure, it would be curious to trace the connection which appears to exist between disease generally—intermittent fever, for example—and the daily atmospheric variations.†" The reader will see at once that facts countenance these speculations. 2. Has the electricity of the air, or the magnetism of the earth, any influence on vital phenomena? If any, we may infer, *à priori*, that the results would be seen in the nervous system. Now, according to the table, the period of increased excitement in the insane commences when the electric tension of the air, and the variation east of the magnetic needle are at a minimum, and *vice versâ*. The unpleasant influence of thunder-storms is well known to persons of a nervous temperament, and to those predisposed to disease of the nervous system, and as these occur most usually in the evening we should look for nervous attacks at that time. While writing I took up the fourteenth volume of the "Edinburgh Medical and Surgical Journal," and found in it twenty-four cases of apoplexy, detailed by Dr. Abercrombie. The time of attack is mentioned in seventeen; ten of them took place in the evening, seven in the morning. A correspondent of the "Magazine of Natural History" refers a curious sound *heard at night*, previous to a change of weather (and which has been attributed to some unknown bird), to "that peculiar singing in the ears which often precedes a change of weather, and is caused by the alteration in the weight of the atmosphere (electric tension?) and the diseased state of the auditory nerve. I have heard it in almost every variation of sound, and I have noticed that the very high notes usually prognosticate rainy weather,"‡ &c. An easterly wind always aggravates the pain in neuralgic affections. I have had a patient with facial neuralgia, who, by his sensations alone, could detect a very slight change in the wind to the east during the night. It also induces a relapse in those who have already had ague. Mr. Barlow, when making observations on the variation of the needle, noticed on several occasions that a cold, cloudy morning, with the wind easterly, was very unfavourable to the increase of the easterly deviation.† At Buenos Ayres the north wind is the general disturber; it puts everybody out of humour, and makes people so quarrelsome that the police anticipate a greater number of cases of quarrel when it blows. Sir W. Parish says that the better classes shut themselves up in their houses while it prevails to avoid mischief. In women it occasions headaches. It also turns meat putrid, curdles milk, and spoils the bread. Everybody complains, and the only answer returned is, "Senor, es el viento norte." The proof that this state is connected with electric changes is, that it is usually terminated by a high wind, or hurricane, from the south-west. 3. Have the sun's rays any direct influence on animal life? Dr. Prout supposes that the diurnal variations he noted in the consumption of oxygen are regulated by the presence or absence of the sun. Light is undoubtedly necessary to the respiration of plants; they cannot excrete carbon in darkness. M. Edwards' experiments on tadpoles kept in darkness are well known. They were never metamorphosed into frogs, but grew to be gigantic tadpoles. Mr. Barlow observes, with respect to the magnetic needle, that the amount of deviation does not entirely depend upon the intensity of the sun's heat, but rather on the intensity of its light.‡ 4. What is the nature of the connection between the diurnal esoteric changes in vital phenomena and the exoteric influences just analysed? On this question I would remark, that we must first understand *the law of habitual recurrence* of vital phenomena before an answer can be given in the least degree satisfactory. Indeed, the whole subject is too imperfectly known, and the facts are too few, and those few too incorrect, to admit of an attempt at analytical comparison. I have examined many volumes for cases, in hopes of finding exact dates, but with the most meagre results. Some of the facts bearing on the question are quite unexplained by

* Works, 8vo., London, 1788, p. 12—18.

† Experimental Researches on the Influence exerted by Atmospheric Pressure on the Progression of the Blood in the Veins.

* Vol. vi., p. 185.

† Transactions of Royal Society, 1823, p. 351.

‡ Transactions of Royal Society, 1823, p. 336.

any hypothesis extant. For example: there are several flowers which (like several species of insects) are perfected without reference to the light or warmth of the sun, and only at certain hours of the day. Linnæus, in his "Philosophia Botanica," divides the day-blooming flowers into three classes, the meteoric, tropical, and equinoctial. Those of the first class are influenced as to their blooming by the state of the weather; of the second by the length of the day; but those of the third open and shut at a fixed and absolutely certain hour. From the habits of these last Linnæus formed his *Horologium Floræ*. If a *mesembryanthemum* be placed in a window facing the east, so that it shall have the morning sun full upon it until twelve o'clock, it will, nevertheless, remain closed until two o'clock, when it will open, although the sun's rays are off it. The observations of Reaumur on certain ephemeræ (quoted in my last communication) are quite analogous; neither cold nor rain retarded their exclusion from the pupa during the hours at which they exclusively broke forth, namely, from eight to ten o'clock, p.m. We can only explain those facts on the suppositions, either that plants and insects are governed by habit like ourselves, or that there is some external influence guiding their movements, of which we are quite ignorant. Both, indeed, may be in operation. All phenomena of this nature are exceedingly worthy of the attention of the philosophic naturalist, and present an absolutely novel field of research.

Having thus noticed the exoteric influences recurring at the lesser periods, I shall next review those recurring at the greater, and examine the question of lunar influence.

York, Feb. 27, 1843.

WHAT ACTS ARE ESSENTIAL TO CONSTITUTE RAPE?

To the Editor of THE LANCET.

SIR,—I was lately requested by a respectable medical practitioner to examine the person of a child, of fourteen years of age, upon whom it had been stated that a rape had been committed by her own father. My examination was made on the fourth day after the supposed occurrence, and observing no appearance of bruise about the vulva or parts adjacent, and also distinctly perceiving an unruptured hymen, which I carefully raised on a probe, to be fully satisfied, I ventured to pronounce a positive opinion that no penetration had been perpetrated; and in this I could not be mistaken, as the parts of the child were remarkably small for the age, and could not by possibility have admitted an ordinary-sized male organ, in a state of erection, without most extensive laceration of the hymen. On the trial, which took place at the Old Bailey last Thursday week, two highly respectable medical witnesses stated their belief that the crime of which the prisoner was accused had been perpetrated. I have no exception to offer to the opinion which these gentlemen had formed, because I know them both to be men of integrity, as well as of experience, and I have no doubt that the two apparently opposite opinions can be reconciled; but I wish to direct your attention to the, at present, imperfect legal definition of "rape." Putting aside, entirely, the question as to the emission of the seminal fluid of the male, the point is this:—In what does the crime consist? And if it be answered, "In the penetration by the male organ," then the question arises, In what does penetration consist? And in the reply to this arises the difficulty. If penetration implies merely the introduction of the male organ as far as the vulva, and does not demand for its completion the introduction of the organ into the vagina, then I am free to admit that in the case under consideration the offence was fully proved; but if the latter condition be requisite, then I positively deny—this *not* having been accomplished—the commission of the offence. I should much like to know the precise legal definition of *rape*, according to the most recent Acts of Parliament. Any medical practitioner may be called upon for his evidence in these most unpleasant cases; and, although we, as medical men, are only bound to answer questions put to us by judge and counsel, and are not required to know much about the legal points of the case, it does appear, from the obscurity of the questions presented to us, that a knowledge of the strict letter of the law may enable us to render assistance, even to the learned judges themselves. Now, if it be required to prove the fact of *vaginal* penetration, as I suspect from the questions put to me by the counsel for the prosecution, and the judge in the case, I think the law is undoubtedly faulty, as it is well known that very many instances are on record of impregnation having taken place, the hymen remaining entire; and an innocent female may be subjected to the inconvenience resulting therefrom, without the ability to obtain redress by prosecution; or, at the utmost, the charge of assault alone could be maintained against the offending party.

The subject is attended with difficulty, but as medical testimony, whether positive or negative, does not rank very high in the opinions of some persons, I have thought it my duty to request to be enlightened upon the subject. I am, Sir, your obedient servant,

JOHN ADAMS.

London Hospital, March, 1843.

PHOSPHORUS may be preserved from the action of the air by a little of the vapour of the oil of turpentine, ether, &c.—*Professor Graham.*

ALLEGATIONS RESPECTING THE CURE OF DISEASE BY WATER.

To the Editor of THE LANCET.

SIR,—Towards the latter end of last year, at the earnest desire of several gentlemen, friends and patients of my own, I went across the Continent for the purpose of examining with my own eyes, and with the best ability I could bring to the task, into the real merits or the demerits of that much-disputed method of treating disease known in this country by the very absurd term, "hydropathy." I took three gentlemen with me, and remained there the whole winter, having but now just returned. While there, I devoted every hour of my time to a personal examination into the cases (more than two hundred in number) which were then under treatment at Gräfenberg, watching daily the effects produced on each case individually. With the view, also, of ascertaining the various effects which could be produced on the condition of the body, in altering the living actions, by the application, in various ways, of cold water, I submitted myself and my three friends to the several modes of treatment adopted by Priessnitz, at his establishment, noting the effects produced on each of us.

I shall not, however, risk your rejection of this letter by burthening it with details. My sole object is to arouse the medical men of this country to a sense of their own interests, and to warn them against suffering a most important branch of medical treatment from falling into the hands of incompetent persons, to whom, also, it does not legitimately belong, thus depriving themselves of their own proper emolument, and the public of much of that advantage which this treatment, *in proper hands*, is unquestionably able to afford. To do this, it will, I trust, be sufficient to state generally the impressions which my observations at Gräfenberg have left upon my mind, and to mention two or three cases, out of a multitude, which I there saw treated and cured. I have seen *complete* fistula in ano cured; a fistulous opening into the ureithra, of two years' standing, cured; epilepsy of four years' standing, cured; hemiplegia cured; deafness, of ten years' standing, cured; and a gentleman who had been competely bald for fifteen years, and who was undergoing the treatment for another disease (a cerebral affection which had rendered the patient perfectly fatuous) recovered both his health and his hair in a few months.

It is in vain, as well as foolish, to stigmatise a treatment which can do these things by the opprobious epithet of "quackery." No kind of *successful* treatment can justly be called "quackery" merely because it happens to differ from the prescribed routine. "Quackery," and "extra-professional," are by no means synonymous terms. To constitute quackery there must be *pretence without efficiency*; and my impressions with regard to the water-treatment are that it is, in the highest degree, efficient, and, moreover, strictly philosophical, and in perfect accordance with the views which are entertained by the most scientific men of the present day (Liebig, for instance) with regard to the true nature of life and disease, and also with that fundamental principle which is laid down by all our best medical teachers, viz., that "all diseases must be cured by the *inherent energies of the living system*, and that medicine can do no more than place the body in the most favourable circumstances for resisting disease."—(Introduction to Dr. George Gregory's Theory and Practice of Medicine.) I have the honour to be, Sir, your obedient servant,

EDWARD JOHNSON, Surgeon.

Nelson-square, London,
March 8, 1843.

FATAL HÆMORRHAGE FROM WORMS IN THE BLADDER.

By J. M. WADDY, M.D., M.R.C.S.L., Birmingham.

I was called up in the night of June 14th, 1841, to visit Mrs. H., a poor woman, residing in Deritend. I found her suffering from the effects of violent and long-continued attacks of hæmorrhage. Her age was about sixty. She was large made, and of leucophlegmatic temperament. Her countenance was blanched, and a large dark-green and brownish circle surrounded her eyes, as in the worst cases of chlorosis. She had lived rather freely, and complained of pain in the right hypochondriac region. Her liver was considerably enlarged. On examination per vaginam, I could discover no lesion of the uterus; and upon strict inquiry I was led to suspect that the discharge came from the bladder, her urine occasionally containing a considerable quantity of blood, although at other times it was perfectly clear and colourless. She was also labouring under deafness, tinnitus aurium, headach, restlessness, and occasional attacks of hysterical epilepsy, arising from the formidable loss of blood.

I prescribed various remedies without being able materially to mitigate her sufferings. In the early part of July I discovered a worm in the urine, dead, gorged with blood, about ten inches long, of the thickness of a goose-quill, with a long and extremely sharp-pointed tail, exactly resembling one of the rat-tailed larvæ, but in a state of great enlargement. I immediately determined to inject the bladder with oil and lime-water, and on the 8th of July I obtained the patient's consent to the operation. Several worms of the same kind were discharged, and the patient remained free from recur-

rence of the hæmorrhage for several weeks. The bleeding, however, returned, and was again and again removed by the injections, worms of the same appearance being discharged, some of them alive, but the greater part of them dead. The patient, however, never properly rallied. The exhaustion from the loss of blood, superadded to the disease of the liver, brought on œdematous swellings, and persistent and uncontrollable diarrhœa, &c., and for several of the latter months of her life she might have been said to exist rather than to live. She died in December, 1842, but I was unfortunately unacquainted with her death until after her funeral; the body, consequently, was not examined, a circumstance which I exceedingly regret.

I have to observe that the examinations of the urine and the injections of the bladder were all made with the greatest care, so as to exclude any possibility of doubt as to the *habitat* of the worms. How they came there is a question. I am inclined to the opinion that they made their entrance from without, a supposition not at all contradicted by any unusual habits of cleanliness upon the part of the patient. There are a few similar cases on record. I believe that it is not impossible that some anomalous cases of hæmorrhage, supposed to be either uterine or the result of vesical calculus, &c., may originate from worms. The instantaneous and beneficial effects of the injection were remarkable, and I have no doubt that she would have recovered had not she been the victim of other diseases.

Birmingham, March, 1843.

CASE OF WATER-STROKE.

By James M. Adams, Esq., Surgeon, Glasgow.

May 22, 1842.—This morning, at five o'clock, a.m., I was called up to visit a female child, aged five months, which I was told had been put to bed the night before without exhibiting any symptoms of indisposition, but had awoke about an hour previous to my being called, in an alarming and apparently dying condition.

On examining the child, which was lying in a state of coma, I observed that the anterior fontanelle was considerably elevated, the face blanched, the lips livid, the eyelids closed, the pupils contracted, and but little sensible to the stimulus of light; respiration was hurried and laboured, and the pulse rapid and small. There were no twitchings of the muscles, nor other symptoms of convulsions present. I ordered a brisk cathartic of calomel and scammony, to be given immediately, the lower extremities to be immersed in hot water, and a turpentine enema to be administered as soon as it could be got ready, and to be repeated in an hour or two if necessary. The little patient rallied, and cried a little on being placed in the warm water, but when taken out of it relapsed into her former state of coma.

No subsequent treatment seemed to have any effect in relieving the symptoms, and at nine, a.m., she expired, being four hours after I saw her, and five hours after the first appearance of illness.

Inspection Twenty-seven Hours after Death.—Considerable effusion of turbid serum diffused over the brain under the arachnoid. The vessels of the brain generally were congested. The ventricles contained no abnormal amount of serum. An unusual quantity of fluid was found in the pericardium. The right cavities of the heart were empty; the left were gorged with blood. The pulmonary artery contained *florid uncoagulated blood*, the aorta was occupied by *dark clots*. The other parts of the body presented nothing worthy of note.

The pathological appearances briefly enumerated, and the short history I have given of the case, coupled with the age and previous condition of the patient, the sudden character of the attack, and its rapidly fatal termination, point to a variety of cerebral lesion by no means common, and presenting a fair subject for inquiry.

The case affords a good example of the hyper-acute hydrocephalus—hydrocephalic apoplexy—*wasserschlag*, or water-stroke, first described by Goelis, and, indeed, with one or two exceptions, not alluded to by any other author. He describes the disease as consisting in a *sudden* effusion of fluid within the cranium, occurring either idiopathically, or the consequence of the repelled matter of a previous disease, or obstructed evacuation from an excreting organ, from which death ensues in a few hours. In all these cases of sudden death there is found, on examination, an effusion of fluid in the head, for the most part in the ventricles of the brain itself

The stages of turgescence and of inflammation are wanting which distinguish this form of disease from acute hydrocephalus; and, again, the stages of transudation and paralysis run into each other, so that the two first cannot be detected, while the two last can scarcely be distinguished from each other. Sometimes, however, as in the foregoing case, traces of inflammation are more or less distinct; the serum effused is less clear than in hydrocephalus, and it is said to contain much less coagulable lymph. The disease commonly occurs in the advanced stages of the *exanthemata*, after the repulsion of chronic eruptions, &c., and in all such cases where the powers of life are much exhausted. Indeed, Dr. Copland says that though he has met with many cases of its *consecutive* occurrence, he has met with none that could strictly be called *idiopathic*. The present case, however, is one strictly idiopathic, for though prematurely born and

of small size, the child had enjoyed perfect health up to the date of the illness which proved fatal.

It appears, then, that this disease is to be distinguished from acute hydrocephalus by the absence of *remissions* or *intermissions*, and by its rapidly fatal termination. The diagnosis is so far of consequence, that we are warned against indulging the relatives with fruitless hopes of the possibility of saving the patient; for, as the affection is *invariably fatal*, and indeed commonly so before the most active remedies, applied at the earliest period, can have time to produce any beneficial effect, the treatment indicated can only be palliative, and adopted to prevent impressions that nothing has been done or attempted for the patient.

Before leaving this case I wish to direct attention to the appearances discovered in the heart and great vessels, as they may have a bearing on the case, which I have failed to detect. The *right* cavities of the heart were *empty*, while the left were gorged with blood. The pulmonary artery contained fluid *florid* blood, while the aorta was filled with *dark clots*. There can be no fallacy regarding these facts, for my brother, Dr. Alexander Adams, who assisted me, was as forcibly struck with these singular and unusual appearances as I was myself. The difference in colour was very remarkable.

Rose-street, Garnet-hill, Glasgow,
March 16, 1843.

THE SALIVA, ACID.

To the Editor.—Sir: In a late number of THE LANCET a quotation is inserted from M. Budge, to show that the saliva is invariably alkaline. He experimented on brutes. I have been in the habit of testing human saliva, and found it generally acid, almost constantly so, in those who have decayed or neglected teeth. It appears to be either alkaline or neutral as it flows from the salivary duct, but, like several other secretions, it speedily becomes acid under the action of the air, or in contact with a putrid substance. The carious part of a tooth is always pretty strongly acid (lactic), because putrefaction is always going on there; and hence the tendency of the caries to spread and infect other teeth, so that the modern tooth-powder, camphor and chalk, seems a good one. Your's, &c.,

G. MOORE.

Hastings, March.

AGENTS INFLUENCING CAPILLARY CIRCULATION.—M. Poiseuille states, in a memoir before the French Academy of Sciences, that alcohol, acetate of ammonia, and nitrate of potass, accelerate the circulation of fluid in the capillaries, both of dead and living tissue.

THE LANCET.

London, Saturday, March 25, 1843.

SOME further considerations on the subject of insanity, in its relations to jurisprudence, induce us again, this week, to advert to the study of *mind*, both in its healthy and its disordered conditions, as forming part of the science of medicine.

It is a singular thing that although medical practitioners have, from time immemorial, been referred to by courts of justice to determine whether individuals were of sound intellect or otherwise, no class of men addicted to learned pursuits has possessed fewer opportunities of studying the subject of mental philosophy clinically. The wards of all our great public asylums for lunatic patients have been wholly closed against them as students of insanity. This fact is deeply to be regretted on many accounts; because medical practitioners are the very men whose peculiar knowledge, and habits of observation and reflection, would eminently fit them to deal with the phenomena of the mind, at the same time that a more careful study of those phenomena, in connection with that of the material organs through which they are manifested, would tend both to enlarge our views of pathology and improve our practice in the treatment of insanity and other forms of disease involving disturbance of the mental faculties.

From the fact that the philosophy of the human mind has been almost wholly uncultivated by those who are best fitted for its pursuit, the study has received a wrong direction, and become a subtle exercise for lawyers and casuists, and abstract reasoners, rather than a useful field of scientific observation. Accordingly, we find the views, even of the most able and clear-headed metaphysicians, coming into frequent collision with the known facts of physiology and pathology. For example,

that "consciousness is *single*" is an axiom among the mental philosophers, and the proof of *personal identity* is made by those gentlemen to rest chiefly on the supposed universality or certainty of that allegation. But what would they say to the case of a somnambulist who evinced what is regarded as a double consciousness—the operations of the mind being perfectly distinct in the state of somnambulism from its developments in the wakeful condition? With reference to such an individual, the proof of *his* personal identity must rest with others, not with himself, for his memory in one state takes not the smallest cognisance of what he thought, felt, perceived, said, or did, in the other.

In the actual state of science there are, we conceive, two principal directions in which the medical profession have it in their power to throw light on the combined study of mind and organisation.

The *first* is an inquiry into the real relation in which the brain stands to the manifestations of mind. This having been a point of discussion among physiologists and metaphysicians for some thousands of years, it may seem strange now to moot it as a matter of special investigation. But the truth is, that the inquiry has not generally been viewed in a just light. The chief point at issue has been converted into one of *materialism*, or *immaterialism*,—whether the aggregate of those phenomena which we consider to be manifestations of "a mind," result from actions of the brain, considered as a living organ, or from the operation of an immaterial thinking principle which uses the brain as its instrument. Now, to agitate this question in the present state of knowledge is entirely futile, because the question is utterly incapable of solution. It runs precisely parallel with the inquiry whether *life* be the result of *organisation*, or *organisation* the consequence of *life*,—an inquiry which all judicious physiologists have, for the present, laid aside, as one on which they might argue for ever without becoming a whit the wiser. In debating the phenomena of mind, we are brought into contact with objects which are purely abstract, and of which physical science can take no cognisance. It would be absurd to speak of the specific gravity of an idea, or of the chemical affinities of a passion, or of the colour of a desire, or of the electrical relations of a resolution. Yet there is no doubt that we *conceive* ideas, are *agitated* by passions, are *impelled* by desires, and *frame* resolutions. These events are as real, and constitute as essential a part of our existence, as does the circulation of the blood or the secretion of bile; and although they are neither amenable to physical laws nor subject to ocular examination, they are controllable by the influence, and subject to the analysis, of the unknown power which produces them. We can weigh our ideas in the balance of our reason, and determine on which side the scale preponderates; we can judge whether the impulse of our passions and inclinations be towards good or towards evil, and can command them to either; and, by moral firmness, we can infuse into our resolutions an almost irresistible energy. Yet, however independent of matter these abstract phenomena may at first seem to be, experience teaches us that the smallest lesion of the cerebral functions may confound the whole majestic series; reduce the strongest and best regulated intellect to a chaos of absurdity, and the loftiest will to utter imbecility of purpose.

The consideration, then, of such results, belongs specially to physiologists and pathologists, who have to regard man as a complex being, made up of body and mind. The accurate classification of mental phenomena, involving, necessarily, precise and logical definitions of the terms by which they are to be designated, is absolutely essential to the successful study of psychical medicine. The necessity of such precision, in thought and language, may be strongly illustrated by a reference to the greater part of our medical writings on subjects in which mental phenomena are concerned, wherein the vague and inaccurate use of terms con-

tinually evince the absence of distinct conceptions in the minds of the authors.

But, admitting that the brain is to be considered as the material organ of the mind, while the mental phenomena are to be regarded, classified, and reasoned upon as abstract results, this physiological question presents itself:—Does the brain, in the production of the mental phenomena, act as an unit, or, like the rest of the nervous system; is it partitioned into separate organs, each possessed of its peculiar vital endowments, and ministering to a peculiar function? The latter view of the subject which has floated, here and there, through the speculations of physiologists since the days of ARISTOTLE, was revived and systematised by Drs. GALL and SPURZHEIM, constituting the modern doctrine of "phrenology." Against this doctrine various metaphysical and ethical objections have been raised; but those objections are entitled to no consideration whatever, considered in opposition to a theory which must be established or disproved by *actual observation*. If, however, any important truth is to obtain for itself universal belief by following out the principles of GALL, a much more rigid method of inquiry must be pursued than has hitherto been adopted by phrenologists. At present a number of organs, the identity of which is imperfectly ascertained, are adduced in support of one another, according to a method of demonstration which would permit us to prove *anything*, and by which, therefore, we *prove* nothing. But let the existence of each individual organ be separately ascertained, by the exact observation of a very large number of cases in which the faculty, or quality, represented by that organ is decidedly present, or absent; then, and not until then, shall we be warranted to bring the facts in relation to that organ to bear upon and illustrate the action of other organs. For example, suppose we want to know whether a particular portion of the brain really be, as is alleged, the seat of *benevolence*, we should make our observations on persons in whom that attribute is either very prominent or deficient. The majority of people are neither good-natured nor very ill-natured, and have what is called the "organ" of benevolence neither very large nor very small. In such cases a reference to the surrounding organs will be sufficient to bear out the bump of benevolence. But such cases, although as good as any other for exemplifying the application of phrenological principles, *when established*, will never suffice as the basis of an inquiry into the truth of those principles, or to confirm the actual existence of any one of the organs concerned. If we find the organ of benevolence to be very large in a man who is generally acknowledged to be remarkably kind-hearted and humane, we should set the case down in favour of the organ; but if, in such a man, we find the organ small, we should set the case down against the organ; nor should we listen to phrenologists who would tell us to observe the large development of conscientiousness, and the entire absence of destructiveness, in such a case. Conscientiousness and destructiveness may be investigated in their turn, but at present we must simply set down the first fact observed, as one that is unfavourable to the claims of the organ under consideration. When the organ of benevolence has been thus tested, by different observers, through many thousands of cases, and it has been found that in a vast majority of very kind and good-natured individuals the organ is decidedly large, and that in a vast majority of those whose character is obviously deficient in benevolence the organ is decidedly small, the fact may be considered as established that the organ of benevolence has a local habitation as well as a name, and *then* we shall have a right to take it into account when reasoning on the action of other organs. In this way, should every organ be separately tested, and until this have been done phrenology will have made no effectual progress, and any important physiological truths which it may involve will lie buried under a heap of superficial observations and illogical inferences. Should the connection of certain powers of

the mind with particular portions of the brain be once established by sufficient observation, an immense field of pathological inquiry would be opened, and our knowledge of mental diseases would acquire a high degree of precision and applicability.

The *second* inquiry which we would suggest relates to the modification of the nervous actions of parts by directing continued and earnest attention to them; or, in other words, to the local effects of concentrating the sensorial energy on a given part. This subject has received very little attention from pathologists, although its bearings have lately been ably pointed out by Dr. HENRY HOLLAND. To the influence of the sensorium on distant parts of the body, a number of singular phenomena, vaguely attributed to imagination, appear to be referrible. Nothing can be more unsatisfactory to an intelligent inquirer unacquainted with such subjects than to be informed by "a doctor" that a phenomenon which he has actually witnessed, and which has excited his wonder, was merely the effect of "imagination" on the part of the individual in whom the phenomenon occurred. For example: a nervous patient says that he has a severe pain in a given part,—a mesmeriser lays his hand on the spot, which in a few minutes is stated to have grown exceedingly warm, when, the hand being removed, the pain is said to be gone. Such an occurrence has often been exhibited to those who have witnessed the proceedings that are ascribed to what is foolishly called "animal magnetism." If, in such a case, an intelligent bystander were to ask a medical practitioner what was the rationale of the "curious fact," and the latter answered "Oh! mere humbug; it is nothing but imagination," the inquirer, in accrediting the phenomena themselves, would conclude that the doctor's "explanation" had explained nothing, and perhaps admit the existence of the mesmeriser's alleged "magnetic fluid." But suppose the medical practitioner were to answer, "These things are difficult to explain on account of our imperfect knowledge of the nervous functions; but it is probable that the hand of the operator acted merely by fixing the patient's attention powerfully on the part, and thereby so concentrating the influence of his brain on the nerves of the part, as to produce a sudden change in their action." The inquirer would here find something like an idea to seize; and although it might seem less distinct to him than to a physiologist, he would probably prefer it to the "explanation" that referred to the agency of a fluid which was only *presumed* to exist, or to the incomprehensible influence of "imagination." That the brain really does exercise on the local nerves a power like that which is above indicated, is sufficiently proved by a variety of phenomena. A person who is waiting with intense anxiety for the arrival of another, from whom he expects to receive deeply interesting intelligence, will sometimes not only identify the individual, but read the expression of his countenance at a distance at which, in his ordinary state of mind, he would not have distinguished him. The sense of hearing is susceptible of a similar exaltation from like causes, and fear, or anxious suspense, or joyful expectation, will so bring the sensorium to bear on the auditory nerve, that a sound will be distinctly heard which would be inappreciable in the ordinary condition of the ear.

The subject of the effects of fixed attention and concentrated sensorial energy on different parts of the body, involves the true explanation of many extraordinary phenomena in insanity, hypochondriasis, hysteria, and other nervous affections, and is, consequently, highly worthy of the attention of all who wish to study, philosophically, the reciprocal influence of mind and body, as well in the maintenance of the normal functions as in the production or cure of disease,—a study which, however neglected, is one of incalculable importance to both metaphysicians and medical practitioners.

THE present Number closes the first volume of THE LANCET for the year 1842-3. Let us take the opportunity of looking back and briefly noting the progress which has

been made in it in medical affairs since its commencement.

This volume, then, holds good a claim to have been an active agent in the advance of medical science, by presenting to its readers, in a concise and comprehensive form, those great principles of animal chemistry on which an improved system of practice will hereafter be reared. Mr. ANCELL's exposition of the views propounded by LIEBIG, with his own valuable additions in the shape of many valuable tables (pp. 384, 667) and illustrative criticisms, may be justly regarded as a valuable acquisition to medical literature. We trust soon to record future observations of this gentleman in the same path of research.

The labours of Mr. Ross have elicited views which bear a close relationship with the foregoing, and the useful and compendious lectures of Mr. ERASMUS WILSON, sufficiently indicate the effect which an improved state of anatomical and physiological science has had in removing, from the extensive class of cutaneous diseases, a considerable portion of the obscurity in which many of them had, until recently, been involved. Among other contributions of much interest in the present volume we may particularise the elaborate papers of Dr. LAYCOCK on the Periodicity of the Phenomena of Life; those of Dr. WATTS on Diseases of the Ganglionic Nervous System; of Dr. WRIGHT on the Saliva; Mr. STEVENS on the Spleen; Mr. GIRDWOOD, on Menstruation, and Sir GEORGE LEFEVRE's valuable remarks on the good effects upon the organs of respiration which might be secured to the people of this country by imitating the precautions against cold adopted in Russia, as calculated likely to attract especial attention.

Care has been taken by us to notice the most interesting recent discoveries and observations in medical science reported in the continental medical journals. The reports of papers and discussions occurringat the medical societies have never been equalled in completeness and interest during former seasons, and the communications on various medico-political and domestic matters have been, thanks to our numerous correspondents, in a high degree useful and interesting. The controversy respecting the physiology of the spleen has occupied considerable space, not without utility, for discussion is a most efficient means of establishing truth.

Removing out of the high regions of scientific inquiry into the bustle of the world below, the general reader has found us in the present volume still combating the narrow aims of the corporate medical bodies, and advocating for the profession those rights to which they are justly entitled as members of a special community, advocating on their behalf the establishment of ONE FACULTY, and a representative government in medicine, as the two things needful to that end; at the same time warring against and exposing to general indignation the fraudulent and injurious proceedings of quacks, both in and out of the profession, and not without some success. Indeed, the annals of quackery scarcely afford an instance of more perfect crushing of a noxious animal than characterised our new year's warning to those unfortunate blind persons who used to wander to the now deserted hall of ALEXANDER TURNBULL, in Russell-square.

Our table is already laden with valuable materials towards the formation of the new volume.

Observations on the Extraction of Teeth; with Plates. By J. CHITTY CLENDON, Surgeon-Dentist. London: S. Highley. 1843. pp. 80.

THIS little book, written in an excellent spirit, deserves to be examined by every person who gives advice on diseases of the teeth, or operates upon them for their removal. We have never seen a more satisfactory literary production devoted to a single subject (the extraction) from the pen of a dentist. Nine out of ten such works are nothing but discreditable advertisements and puffs. In the present case we have a sound practical work, evidently the result of a simple and sincere desire to be useful in the art, not hastily produced, but based on careful reflection and experience, and well meriting the best character that can be given to it by a medical journalist.

MEDICAL SOCIETY OF LONDON.

PECULIAR CONDITIONS OF THE OVARY.

MARCH 6th.—The PRESIDENT (Mr. Pilcher) exhibited to the society the ovaries of an unmarried lady, fifty years of age. These ovaries presented a remarkable appearance; one of them was about the size of a goose's egg, firm and hard in consistence, and on cutting it open it was found to contain a fatty saponaceous matter, which was covered by the tunica albuginea of the organ;

the other was somewhat smaller, but contained the same kind of matter mixed with hair. The uterus was hard, firm, and fibrous, and neither this nor the ovaries had ever given evidence during life of being in any way diseased.

Dr. BIRD did not consider these cases uncommon. He had seen two cases the previous week in Guy's Hospital in which the uterus contained, in one instance, about a pound and a half of a butter-like matter, mixed with hair and teeth, and in the other this organ contained about a pound of the butter-like matter without the hair and teeth. In the case of a woman who had lately died under his care with symptoms of *gastro-enterite*, the ovaries presented a somewhat similar appearance to those before the society, only that they were smaller, one being of the size of a hen's egg, the other of that of a walnut; both contained fatty matter like those in Mr. Pilcher's case, and in one there was also hair. This matter was ordinary fat, but contained a rather larger quantity of stearine than usual, which gave it a glistening appearance.

Dr. WILLSHIRE suggested that the matter in these cases was similar to that found in the sebaceous follicles in acne, and in the enlarged follicles of the scrotum in dirty old men.

Dr. RISDON BENNETT had been shown this kind of matter in enlarged sebaceous follicles as far back as 1830, by Dr. Sharpey. It was always to be found in the enlarged follicles of the face. He recollected a case in which a patient died of purpura, and on examining the body after death the ovaries were found much enlarged; one of them was like an enlarged cyst, and contained a semi-fluid matter something like tuberculous matter, and of an ochre colour.

Dr. LEONARD STEWART referred to a case which had been exhibited to the Westminster Society some years since in which an ovary contained both hair and teeth.

ANNIVERSARY MEETING.

Tuesday, March 8, 1843.

This meeting was held, as usual, at the London Coffee-house, Ludgate-hill, and was very numerously attended. Dr. Leonard Stewart delivered the annual oration, the subject discussed in which was

THE PUBLIC HEALTH.

He commenced by adverting to the great influence of climate and locality, and on the necessity of removing from unwholesome situation. He then continued thus:—

"The statistical information which is now being obtained from many quarters on the subject of medical topography, is of the greatest importance in directing the migration of invalids, the route of all expeditions, and the establishment of colonies and new settlements. It occasionally gives a reason for abandoning regions, which are either too severely cold for the susceptible natives of more sunny skies, or insupportably hot and pestiferous to the labourer from the north. It appears that any frozen climate develops tubercles in the Malay and in the Negro; while we are told that the marshy plains of Demerara, though prolific of other diseases to the Anglo-Saxon race, are both preventive and curative of phthisis. There is some reasons for believing that the causes of ague and consumption are antagonist powers, and do not commonly prevail in the same localities. We have thus before us the good and the evil, and can at pleasure use without abusing the natural influences around us."

He then remarks upon the comparative safety of the climate of the British isles:

"When we ascertain that there are fewer malarious and atmospheric causes of malady with us than elsewhere, we at once establish the very important conclusion that the circumstances which generate and diffuse the evils that do exist are, in their essential nature, more accidental and artificial. Our prevalent sufferings are therefore connected, it is probable, with modes of life and social conditions which are not incapable of reform. Such, for instance, as the precocious labour of children and the sedentary occupations of females. There is a great deal that can be traced to what is familiar and tangible, as to dirty and insufficient clothing, damaged provisions, over-crowded rooms, excessive toil, intemperance, often to ignorance and prejudice. We cannot for a moment doubt that by investigating and correcting these and many other improper habits of a similar character, much unmixed good may be done."

Among other things, the situation of the children of the labouring poor is thus noticed:—

"The forlorn and helpless situation of individuals, or small families, when isolated and at a distance from help and attendance, is to none better known than to medical men. It is well worthy the attention of philanthropists, and those who desire the well-working of the social mass, to contrive some plan for the protection of children when their parents are away. The infant-schools, and other places of deposit during the day, are shut out from them when ill. The parents (whose occupations often keep them abroad) are frequently obliged to employ very inefficient persons to supply their place, or they have the difficult alternative of giving up their work and forfeiting their only means of support. Even where there is no neglect, it is well known to those who are in the habit of attending the poor at their own homes that there is too frequently great ignorance and prejudice, and that those helpless objects suffer from all kinds of mismanagement, injudicious meddling, and caprice. Add to all this the danger continually arising from

the number of persons huddled together in one room, frequently in one bed."

The necessity of the personal interference of medical men was thus insisted on :—

"I am not ignorant of the great attention which has already been given to many of these matters. I rejoice that much more is likely to be done in this direction, but I think it only right to say that much more of the control and management of all sanatory regulations should be in the hands of medical men than has hitherto been common. We have something more to do here than to set other people to work! Our vocation is administrative as well as legislative. We are not abstract advisers, but peculiarly and proverbially *practitioners*. Medical men should be in person ready to superintend the execution of what they may theoretically recommend. We have the highest political authority of the present day declaring that a physician should not prescribe until he is *called in!* To suppose that there is an inherent virtue in formulæ that enables any one without education or practice to deal with them successfully, is really as absurd as to fancy that the colours and brushes of a portrait painter constitute the chief part of his skill. And yet this is common enough!"

The difference between physiological and physical experience was thus stated :—

"In chemical and mechanical operations the action of the substances which meet is mutually balanced, and can be calculated upon. The result is unvaried and certain. There ought to be no irregularity, because these are purely physical experiments—they are of the 'exact sciences.' It is only where the vital energy and organic power is called into play that there is any uncertainty or caprice, or that action and reaction is not reciprocal and mathematically adjusted. This essential difference, it is very clear, cannot be attributed to the brute substance, the passive inanimate material, but at once marks the peculiar character of organisation, the more active, initiative, and independent function of the living fibre. There seems to be a discriminating power, a force which resists and repels the approach of deleterious matters, and their ingress into the absorbing surfaces. Again, it appears that this power is lost or modified, or possibly inverted, so that what was once excluded by the skin and mucous surfaces is, at these times, attracted and absorbed. We continually observe the different susceptibilities of various constitutions, or of the same person, at different periods, and in the many fluctuating conditions of health. We know that neither food, nor medicines, nor poisons, are acted upon in one invariable mode. Now the moral of all such researches is that no routine can be established for all emergencies, because the true agents in all remedial transactions are the organs and functions of the living frame, and not the material appliances which are merely the occasions and instruments, the subject matter of any change. Too much stress cannot be laid upon this doctrine in our dealings with the public."

The importance of a greater dispersion of practitioners into other regions was then noticed :—

"But if an acquaintance with the natural history of disease, and the various sciences which are appealed to and put in requisition by the healing art, is necessary to the safe and successful practice of our profession, it follows that the unskilful employment of recipes by the ignorant, by amateurs, or by the followers of *routine*, is dangerous. It becomes more and more important to supply the personal attendance and assistance of the skilful and experienced as generally and as widely as possible, to spread abroad not only the measures but the *men*. There can be no discovery, no conquest obtained by the skill or enterprise of our expeditions into various regions more truly important than the sure victory established on all sides over the mischievous systems and ignorant practices pursued by various less enlightened people. This, although not always the object, is frequently the best result of such undertakings."

The orator concluded by recommending a more even distribution of the superabundant productions and resources of different countries, and more attention given to the adaptation of the medical profession to direct and conduct emigration and colonisation :—

"We may imagine the many powerful elements of knowledge and production which now jar and fret in opposition, and competition more harmonised and combined in effecting the general advantage. In cities, in rural districts, in distant settlements, and in home colonies, it is still to be seen whether the peculiar good of each is inseparable from the usual drawback of evil to which each is now subject. We may fancy such expeditions as have already occasionally taken place made more frequently and systematically, so as to encourage and hasten the progress of unsophisticated nations from barbarism to refinement. What unnumbered benefits would arise to all, both far and near, from the solution of the great political problem which proposes so to combine the results of intellectual and associated life as to bring them to bear upon the rustic and the savage!"

After the oration was concluded the company, to the number of fifty gentlemen, sat down to an excellent dinner.

OFFICE-BEARERS ELECTED.

President.—G. Pilcher, Esq.

Vice-Presidents.—H. Clutterbuck, M.D.; W. D. Chowne, M.D.; W. Kingdon, Esq.; H. Hancock, Esq.

Treasurer.—N. Clifton, Esq.

Librarian.—W. C. Dendy, Esq.

Secretaries.—H. P. Robarts, Esq.; W. A. Harrison, Esq.

Secretary of Foreign Correspondence.—Thomas Davidson, M.D.

Members of Council.—W. D. Chowne, M.D.; W. T. Elliott, Esq.; F. Hird, Esq.; F. Hutchinson, Esq.; Jas. Middleton, Esq.; J. R. Bennett, M.D.; S. Proctor, Esq.; R. L. Hooper, Esq.; John Forster, Esq.; W. B. Diamond, Esq.; Ed. Headland, Esq.; E. H. Linnecar, Esq.; L. Stewart, M.D.; Theoph. Thompson, M.D.; Golding Bird, M.D.; John Bishop, Esq.; W. H. Willshire, M.D.; Silas Stedman, Esq.; Marshall Hall, M.D.; J. F. Clarke, Esq.

The Fothergillian medal was awarded to Dr. Lever, for an Essay on Organic Diseases of the Uterus; and the society's silver medal to Edwards Crisp, Esq., for his general usefulness to the society.

EXTRA-LICENTIATES.

To the Editor.—Sir: I beg to challenge your correspondent, "Chirurgus," to adduce the name of a single extra-licentiate who has called himself a member of the College of Physicians *knowing that he was not "a member."* The statutes of the college make no mention of the name of "member." There are there described *socii* (or fellows) *permissi intra urbem* (or licentiates), and *permissi extra urbem* (or extra-licentiates); I apprehend, therefore, that it is only as an assumption or as a concession by courtesy that the licentiates designated themselves "members;" and I cannot see on what ground the extra-licentiates can be charged with being "guilty of a fraud," or of "a bold abuse of truth and decency," in adopting a similar style; they undergo the same examination, and, according to the evidence given before the House of Commons, the examination, though conducted in one day, is "equally as strict." "It is, in fact, the same examination." They pay a fee to the college funds, and their names appear in the published "Catalogue of the Fellows, Candidates, and Licentiates of the Royal College of Physicians, London." I maintain, therefore, that these facts are sufficient to justify the use of the title of member on the part of the extra-licentiates. But, were it otherwise, the use of the appellation by an individual could only be attributed to a misconception of the powers the law has given, as it is scarcely possible to conceive an object which any men could hope to obtain by "a fraud," "in print" so open and so readily exposed, as this would be. The essence of turpitude, Sir, consists in the "scienter;" and whether "Chirurgus" be right as to this question or not, I would advise him to cultivate a little more charity, and to eschew language which can only tend to raise doubts as to the veracity of his own signature. I am, Sir, your obedient servant,

AN EXTRA-LICENTIATE.

*** The above letter was intended to follow the communications published last week, at page 910, but was accidentally omitted. The note then added applies *now*, and must be regarded by all our correspondents as a "closing of the account."—ED. L.

JOHANN WILHELM ARNOLD.

(*From a Correspondent.*)

A SILLY publication of this writer has been recently reviewed in Dr. Johnson's journal. The object of the present writer is merely to undeceive the English medical readers, many of whom imagine that *this* Dr. Arnold (Johann Wilhelm Arnold) is *the* Dr. Arnold (Friedrich Arnold), the well-known author of two of the best works on the anatomy of the nervous system of the day. Dr. J. W. A. seems to be in a great fury. A writer in Schmidt's Journal (Jahrbuch) has greatly incensed him by calling his style cold and spiritless, and without light and shade, and Dr. J. W. A. is highly exasperated, apparently, against Professor Müller, whom, with Dr. Marshall Hall, he treats without any ceremony. He deals fierce blows against the theory of the reflex function; but they are, like the blows of the drummer—all noise.

VENEREAL WARTS.

To the Editor.—Sir: In answer to your correspondent, "S. C.," page 775, who wishes to hear of some applications that have proved effectual in the removal of venereal warts, I beg to state that I have found the *alumen exsiccatum*, or the *liq. plumbi diacetatis*, adequate to the cure of recent ones, but that those of long standing are better treated by the application of *concentrated acetic acid* (such as is used for aromatic vinegar), or, in cases of extreme induration, the *chloride of zinc*. I have yet to see the case that will not yield to one or other of these remedies. I am, Sir, your obedient servant,

JOHN CRAMER CHAPPEL,
Formerly House-Surgeon to the Lock Hospital.

Jermyn-street, March, 1843.

HYDROPHOBIA AFTER TWO YEARS.—A remarkable case of hydrophobia occurred last week in Newcastle. The sufferer, a labouring man, Richard Oxley, about two years ago was bitten by a mad dog in two places on the left arm. The wounds were dressed by a surgeon, and in due course healed. No further inconvenience was felt by Oxley

until the 11th March, 1843, when he suddenly became ill, and during the following day manifested decided symptoms of this disease, falling into violent fits whenever any liquid was presented to him. Medical assistance was procured, but without effect; he expired in great agony on Tuesday morning. —*Newcastle Paper.*

ROYAL COLLEGE OF SURGEONS IN LONDON.

List of gentlemen admitted members on Friday, March 17, 1843:—P. Hubbert, T. A. Warren, W. Williams, T. Hawkins, P. A. Boyle, J. B. Davis, J. Allan, J. R. Passe, J. T. Jenkins, J. S. Nott, J. H. Coveney.

BOOKS RECEIVED.

Remarks on Medical Reform, in a second letter addressed to the Right Hon. Sir James Graham, Bart., &c. By Sir James Clark, Bart., M.D., F.R.S., Physician to the Queen. London: J. Murray, 1843. Pp. 40.

The Causes, Nature, Diagnosis, and Treatment of Acute Hydrocephalus. A Prize Essay, to which the Medical Society of London awarded the Fothergilian Gold Medal for 1842. By J. R. Bennett, M.D., &c. London: S. Highley, 1843. 8vo, pp. 248.

Observations on the Efficacy of Traction in the Cure of Consumption and Asthma. By D. Cronin. London: B. Wertheim, 1843. Pp. 65.

NEWS AND CORRESPONDENCE.

A Correspondent at Daventry.—The jury at Bristol were bound to return their verdict upon the evidence, and that they did, although (according to the report) in terms such as could have been received only by a coroner who was ignorant of medicine. The evidence did not specifically connect the disease with any external violence.

To the Editor.—Sir: Permit me to ask the advice of my professional brethren on the following case:—Mary D., aged twenty-two months, has long been afflicted with complete incontinence of urine, which seems to dribble away as fast as it is secreted. The vulva and parts surrounding are constantly sore from the irritation of the urine. Before bringing her to me, her mother had had her under medical care, and at one time she was treated by a druggist. When I first saw her the mother said her bowels were in pretty good order. However, I gave her alterative doses of mercury with chalk, combined with magnesia, and afterwards administered the tincture of muriate of iron, along with tincture of cantharides, keeping the excoriated parts covered with calcined magnesia. Very slight relief has been experienced, but yet her mother says greater than from any other of several methods of treatment. Cold bathing produced no good effect.

In this part of the country (Yorkshire) we have lately had many cases of what the women term "scarlet rash," the principal symptom of which is sore throat, attended with considerable swelling of the tonsils, in relieving which the tinct. iodin. co., applied externally, acts with wonderfully good effect. Indeed, I have found it very useful in acute cynanche tonsillaris, occurring to patients of all ages. As there are many persons thus affected at present, the hint may be useful, though I need scarcely remark that it has no pretence to originality. I am, Sir, your obedient servant,

S.

The letter of *Mr. Edward Johnson*, at page 934, having an honest-enough appearance, filled a place in a sheet of the present number, when we discovered that the author was about to begin to quack on the subject in another place; but the fact was noticed too late to allow the communication to be removed, and we can therefore only append this indication of the rank that the writer should probably hold in the view of the profession.

Mr. Clendon.—The friend did not send the copy.

Visitor.—What is the ground for attacking the party? Sufficient public reason is not given. The general comments on the other subject want the charm of novelty in matter and manner.

The letter on the examinations of ovaria during the menstrual flux (*A. B. C., versus* Mr. Girdwood) came too late for insertion this week.

The letters of *Mr. W. Tayler*, and many other gentlemen not before acknowledged as received, will be inserted in an early number.

The papers of *Dr. Wright* will be continued as soon as we can command space for their appearance.

The Bath case cannot be noticed in our pages this week.

Erratum.—In Mr. Ross's paper on Typhus, page 890, in the first prescription, line 4, for "to be taken every four hours," read, *to be taken in four hours afterwards.*

END OF VOL. I.—1842-43.

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